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MEDICAL RESEARCH COUNCIL

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OBSERVATIONS,

&c.

ON THE LATE

EPIDEMIC DISEASE

AT GIBRALTAR.

PART I.

BY

SEGUIN HENRY JACKSON, M. D.

&c.

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OBSERVATIONS,
&c.
ON THE
EPIDEMIC DISEASE,
WHICH LATELY PREVAILED
AT GIBRALTAR:
INTENDED TO ILLUSTRATE THE NATURE
OF
CONTAGIOUS FEVERS IN GENERAL.

BY
SEGUIN HENRY JACKSON, M. D.

MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS, LONDON;
CONSULTING-PHYSICIAN TO
THE WESTMINSTER GENERAL DISPENSARY;
AND PHYSICIAN TO THE INFIRMARY OF SAINT GEORGE,
HANOVER SQUARE.

Part I

“ Solus veritatis Amor, et communis utilitatis Studium, ad has partes suscipiendas impulerunt. Quid quantumque hoc Meum sit, prudentis, ac boni Viri Judicium esto; Mihi satis superque erit, illud, et publicæ utilitati, et præsertim ANGLORUM CIVIUM incolumitati, consecrasse.”

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1806.

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TO
HIS ROYAL HIGHNESS
THE
PRINCE OF WALES.

SIR,

THE importance of the subject, which the following pages are intended to elucidate, and the interest your ROYAL HIGHNESS has uniformly taken in whatever can benefit mankind, have induced me to solicit, that your ROYAL HIGHNESS would graciously accept them.

The opinions which your ROYAL HIGHNESS has in consequence permitted me to lay before you, I trust will not be found wholly undeserving of notice. They are the result of much reflection, and of many years experience. I submit them to the inspection of your ROYAL

DEDICATION.

HIGHNESS, and of the public, with diffidence;
but at the same time with a full persuasion,
that they are warranted by facts.

That your ROYAL HIGHNESS may long live
to enjoy the loyalty and affection of a free and
happy Country, is the earnest wish of,

SIR,

Your ROYAL HIGHNESS'S

Most grateful,

And

Most dutiful Servant,

SEGUIN HENRY JACKSON.

Hanover Street, Hanover Square,

June 1st, 1806.

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OBSERVATIONS, &c.

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Since the printing the Preface was completed, the Author has been informed, that the Practitioner alluded to at the conclusion of it, has dropped his intentions of publishing on the subject. He learns, also, that that respectable Gentleman was the leading Physician at the Garrison, when the Epidemic Disease broke out, and not sent thither, as the Author supposed, when he forwarded the Preface to the Printer.

THE PREFACE.

THE universally known danger, and even acknowledged fatality, of the malignant epidemic fevers of tropical climates, must stamp some degree of importance on the most humble attempt to elucidate the hidden nature of such dreadful maladies. To this general purpose the author's mind has been devoted in the prosecution of his endeavours, to investigate the particular and specific nature of the late Gibraltar pestilence, by discovering, if possible, the immediate cause of its mortal tendency. In prosecuting these his views, he has met with nothing to shake his hopes of throwing some new light on this hitherto dreadful scourge on the inhabitants of the tropical climates. He flatters himself, that they will in a great measure tend to the security, as well as ease of mind and happiness, of mankind in general.

When we consider the late mortality at Gibraltar, as arising from a disorder of a relative character to those endemic and epidemic fevers, which have been ordinarily denominated the plague, the yellow fever, and the highly malignant typhus, the treatment of the subject must interest all those, who are destined to visit the countries, where such periodical complaints almost annually occur, and also all those inhabitants of the

world, who have a commercial intercourse with such pestilential situations. Writers, both ancient and modern, (for such calamities have been recorded for several thousand years) acknowledge, “that the nature of the plague is as little known, as that of some other diseases.” It would be very lamentable information to mankind, if other diseases were as little understood as the plague. But I may aver, that we are far more advanced in the general pathological science of medicine, when it is compared to what has been hitherto discovered of either the plague, the yellow fever, or all those generally called malignant and pestilential fevers of a similar description.

The BOARD OF HEALTH of this country, consisting of several most respectable members of society, and of the Royal College of Physicians, in their outline of a plan under the sanction of the HONORABLE PRIVY COUNCIL, to prevent the spreading of the plague, or other contagious diseases attended with great mortality, in case such should pass the barrier of quarantine, and actually appear among us, have denominated the late febrile disease at Gibraltar, a *pestilential distemper*, and have contrasted its defined character, as received from thence, with that of the plague. See the First Report 30th April, 1805.

Pestilential distempers, ordinarily understood as such, or in other words contagious fevers, have been considered either as the plague, the yellow fever, and the bilious remittent fever, of tropical climates; or the typhus, and putrid fever, of our own country; terms of distinction applied to perhaps different degrees of virulence only of the same general disease. They have all hitherto been considered as febrile disorders of the high

typhoid malignant character, and of the most pestilential, or pestiferous, tendency. A doubt of the true nature of the typhus fever of this country, long entertained by the author, had for some time led him to reflect much on the general pathology of fever, while treating complaints of that character: and the late remarkable events at Gibraltar determined him to hazard his opinions on the subject before the public; having suspected the mortality at that place to have arisen from a PYREXIAL EPI-DEMY, and not from a truly contagious malignant typhoid disease, as a genuine idiopathic fever. He thinks it can be, by a fair deduction from facts, proved to have been more a local inflammatory affection, and also, properly speaking, not of contagious, but atmospheric, origin.

In the minds of many learned and long experienced practitioners, the opinion respecting the existence of high contagion seems to be losing ground, and will, the author flatters himself, be in due time wholly laid at rest, which must afford considerable comfort and felicitation to ALL, but in a more especial manner to the commercial parts of the world. A great step towards a more minute and steady inquiry into the truth of this part of the subject must be gained, from the new sentiments entertained on it by Dr. RUSH of Philadelphia; and in order to extend as much as possible the publicity of this Professor's new way of thinking, the author begs leave in this place to inform his reader, that the Editors of the Medical and Physical Journal of London have, in their number for March last, namely the 85th, been addressed by Mr. L. J. JARDINE, of Liverpool, in the following pointed manner, as

regarding Dr. RUSH's recantation of his old doctrine of febrile contagion.

“My friend Dr. RUSH, of Philadelphia,” says the correspondent, “has in his new edition of his *Medical Inquiries and Observations*, retracted his former opinion respecting the contagious nature of the yellow fever, and being desirous of making this recantation as public as possible, he has requested me to obtain the insertion of the following extract from his (Dr. RUSH's) preface, in one of the periodical works of this country. If you will have the goodness to give it a place in your valuable *Journal*, my friend's object will be fully accomplished.”

Liverpool, February 10, 1806.

The extract is as follows. “In the fourth volume the reader will find a retraction of the author's (Dr. RUSH's) former opinion of the yellow fever spreading by contagion. He begs forgiveness of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the misery and mortality attendant upon that disease. Indeed such is the pain he feels, in recollecting that he ever entertained or propagated it, that it will long, and perhaps always, deprive him of the pleasure he might otherwise have derived from a review of his attempts to fulfill the public duties of his profession.” *Preface to his Medical Inquiries.*

Nothing could be more honourable than this conduct of Dr. RUSH, who proves himself, by the preceding recantation, to have ever been the true friend and physician to society

The author flatters himself, that the particular opinion he at present ventures to throw out for the consideration of the profession and the public, will prove a fresh ground of support to the doctrine of NON-CONTAGION, and amply justify the learned Professor, in having retracted from his former opinion.

The author suspects much, that during the period of a raging pestilence, or even a sickly season in a tropical climate, disorders of a febrile character, though arising from excess of fatigue, bad provisions, or any other occasional cause, have too frequently been considered as partaking of the truly epidemic nature. Doubts may sometimes be just, even on other grounds. ASSALINI, a late writer on the plague of Egypt, speaks of a few deaths happening from a suspicious disease, which excited alarm, and which, without having recourse to contagion, were, in his opinion, occasioned by the evaporation of stagnant waters, rendered noxious by the putrefaction of aquatic plants, and of unusual quantities of fish. In this opinion he accords with the present sentiments of Dr. RUSH. But I shall find occasion to speak more at large of ASSALINI's experience and observations, during his residence with the French army in Egypt and Syria, in a future part of my undertaking.

It is a secondary object of the present publication, but one of no trifling consideration, to find a path leading to a better understanding, as well as a more successful treatment, of the TYPHUS, or *typhoid fevers*, of our own country. It has become in a manner so much the fashion to admit this GENUS

of fever to be so general, that the one in direct opposition of character to it, namely the *SYNOCHA* or *inflammatory fever*, is now rarely acknowledged to be met with in practice. And because fevers in general in this country are not characterized so strongly with the full or high typhoid symptoms, as they have ordinarily been remarked to be in tropical climates, nosologists have admitted, and practitioners have countenanced, the idea, or existence, of a mixed fever, which has been denominated the *SYNOCHUS*. This is properly the true low nervous fever, which the author is disposed to consider as being most frequently a symptomatic, not a true idiopathic, fever. He will attempt to elucidate this particular sentiment in a subsequent part of his undertaking, in the hope of hastening in future the cure of that particular description of disease, which has been so denominated, and as such treated, and which has so often proved fatal even beyond twenty-one days, notwithstanding the most regular, and scientific, principles of practice. This is also that particular kind of fever, which being treated as of an idiopathic character, has so often been discovered after death to have been dependent on some local inflammation, or visceral affection, which had not been early enough suspected, or properly understood, during the life of the patient. This becomes a most important subject of inquiry.


The author has adopted the plan of publishing his intended illustration of the Gibraltar fever, and of epidemic fevers in general, *IN PARTS*, from the following considerations. I. It will give him the opportunity of sooner making known the particular opinion he entertains, as the *FIRST PART* will discover

the main object of his future reflections, and the purport of the parts, which are to follow. II. It will, he hopes, by thus early throwing himself upon the candour and judgment of professional men, obtain for him the sentiments of others, towards the future prosecution of his undertaking, as he proceeds with it, and which, whether favourable or not, he may embody with the succeeding parts of the publication, as they should happen to approximate to the nature of his own plan. He has already perfected the plan according to his own way of judging of the subject, and has arranged the facts in evidence of his opinion. They will be selected from both medical and surgical practice, as connected with various sensorial affections. The several succeeding parts are also nearly ready for the press. The whole will form an aggregate of observations, communications, commentaries, and conjectures, interspersed with practical remarks, so as in the end to establish a new pathology of the ordinarily admitted contagious and epidemic fevers of every clime.

The first part will embrace general observations on the science of medicine, and on the present opinions of febrile and sensorial pathology. The second part will comprise such communications, with occasional remarks, as have already been received on the particular subject of the late Gibraltar fever. The third part will consist of commentaries on those communications, and on the treatment of the said disorder, in the light both of an ordinary pestilential distemper, and in the way the author has conceived of it, namely, as an epidemic phrenitis, or brain fever. The fourth part will particularly

have in view practical observations on the whole; with indications appropriate to the future prevention and cure of such a truly local, or phlegmasial, disease. Of this plan the profession and the public are to be the judges. To their adjudication it is most humbly submitted; and if by the practice and reflections of others hereafter, the opinions now thrown out should be established, by future trials and experience, the author will feel much satisfaction at having ventured to submit them to the gradual consideration of posterity.

The lapse of time, since the author first intimated to the public his being engaged in an enquiry into the nature of the late Gibraltar pestilence, requires some apology. He wishes it therefore in excuse to be understood, that the delay has partly arisen, from his not having received some communications, with which he had indulged the hope of being favoured: (this may hereafter be more particularly explained :) and partly, from not having so much hastened the printing of the FIRST PART, owing to the constant expectation, and rumour, of an intended publication on the subject, from the pen of an able and intelligent medical practitioner, who had been sent to the garrison to assist at, as well as investigate, the fatal scene.



The reader is requested to observe, that the references by numbers, in the progress of the work, relate to the paragraphs, and not to the pages.

INTRODUCTION.

Preliminary Observations.

1. THE hazard in submitting to the consideration of the learned and long experienced new practical observations on a particular disease, is fully felt on the present occasion. I acknowledge my doubts and the difficulty of conducting the present undertaking with full satisfaction to my readers, from not having had a personal opportunity of judging of the Gibraltar malady, by a close comparison of its rise, progress, and termination, with the history of those affections, to which it has been supposed allied. My only apology, therefore, to the public and the profession, for such an intrusion, must be on the ground of calm reflection, and general experience for a long series of years; having very early in life indulged a desire of investigating some of the *arcana* of fevers. Very soon after my first being in practice at the Westminster General Dispensary, namely, in the year 1780, I entertained an opinion, from particularly attending to the phænomena of pyrexial diseases

in this country, that the real nature of the epidemic and endemic disorders of the Continent had never been fully understood, at least, had not been so fully investigated by the ablest practitioners of the tropical climates, as their specific character and fatal tendency seemed to demand. All my readings on the subject of local fevers in this or that particular quarter of the world, with all deference to my professional brethren, had then been and still remain so unsatisfactory to my mind, as to fall short of explaining to it all the phenomena which have attended them: on the contrary, they have ever impressed it with a feeling of conviction, that the ordinary want of success in the medical treatment of contagious fever, commonly so called, even in this country as well as abroad, arose from its not according with the true nature of the spreading malignant disorder, either with regard to the predisposing, occasional, or proximate causes.

2. I was next led to pay more particular attention to the subject by forming, as I went on, some criticisms on my own practice, for which my attendance at two public institutions, for the last twenty-one years, afforded me the best opportunities, and even had been of no small extent at one of them, for several years before. About a year and a half ago, when my mind became most decidedly bent upon attempting an investigation of fevers on the Continent, from which I had only been so long deterred by the known difficulty of the subject, an event took place in my own practice, (and which will hereafter be mentioned) that so confirmed me in the opinion I had for some time entertained of former epidemics, as to be felt by me, as nearly removing that cloud from before my eyes,

which must have often stood in the way of my own success in practice, as well as it may have been an obstacle to that of others. With the encreasing force of these sentiments, my intentions acquired new zeal at the time the nation became so alarmed by the apprehension of the approach of an epidemic or contagious fever from the shores of Andalusia, lately so destructive to the inhabitants of our valuable fortress of Gibraltar. This painful event has naturally served to lead me to a still closer inquiry into the subject, roused by a fatality to my countrymen which has been unexampled; and which to my reasonings on the melancholy occasion is a proof, that the true nature of the recent disorder at Gibraltar had not been at the time suspected by the most experienced practitioners on the spot, which I think can only be attributed to the prevalence of *one opinion*, and the force of example on all such occasions, in the treatment of almost all epidemic and endemic fevers. This *particular opinion* will be more pointedly noticed hereafter.

3. In order to qualify my mind for full reflection on the subject, and to balance the want of direct personal experience, I have of late read at my spare hours from professional and domestic avocations, such recent observations on that febrile disease called by some the plague; by others, the yellow fever; by some again, the bilious remittent fever; and by many, a pestilential typhus; as would furnish me with a selection of well-grounded facts relative to those tropical disorders: presuming that whatever advantages might have arisen from consulting ancient publications on the subject, must have already been experienced by the later writers, when they had referred

to those authors. But *all* the practitioners of the present day seem to have agreed in one opinion; namely, that the disease, the subject of my particular enquiry, was either a typhus, or a plague, or a yellow fever, or some such disorder, and generally considered of the most malignant kind, as well as of an epidemic and dangerous character: though it does appear, from the observations and hints of many professional and learned men, that the contagious nature of the disease is of late much called in question. It affords me therefore much satisfaction to observe, by the writings of literary practitioners of the present century, that the mind of the medical philosopher has at length become bent upon determining, whether or not contagion is the source of the spreading febrile evil, during a pestilential season any where, as well as in tropical climates.

4. As a proof of the ambiguity attendant on the subject, the learned and experienced Dr. RUSH of Philadelphia, has wholly changed his opinion, since the ravages of the yellow fever in the years 1793, 1794, and 1797, of which he then had such experience, and such opportunities for judging of its rise and progress. He has of late become a strong advocate for the non-contagiousness of the disorder. The Doctor however has not succeeded in bringing over, to his new way of reasoning on this subject, the Editors of the New Edinburgh Medical and Surgical Journal. They are disposed to give more weight to the arguments he formerly made use of to support the doctrine of contagion, and have even defended his former sentiments by some evidence and reasoning of their own. These gentlemen are even afraid of a nation being put off its guard, by giving in to the recent opinions of Dr. RUSH, which are so

well calculated, if hereafter proven to be just, for future benefit to society, not only to families and to whole cities, but even to kingdoms and empires. Consistent therefore with their way of thinking, the Editors have concluded their criticisms on the learned writer's defence of non-contagion, with the following sentence:—"Could any degree of freedom in commercial intercourse compensate for the valuable lives which were lately lost at Gibraltar, in consequence of the doubt or disbelief first entertained of the contagious nature of the disease, which almost depopulated that important fortress!" This I trust, and think, was not the true cause of the subsequent mortality; and as some consolation to those affected by the above opinion, I shall venture to hope, that it arose solely from the *hidden* nature of the disease, whether contagious or not. Such however is the present divided state of opinion on this important subject. But the controversy must in the end lead to the good of the people; and I trust that the question giving rise to it will not be lost sight of, until it is fairly and satisfactorily determined on, one way or the other. The investigation is arduous, and will necessarily be liable to diversity of sentiment, from local and provincial causes.

5. I profess not to have read much, not at least the voluminous writers on the subject; probably not enough therefore in the opinion of some: but I contented myself with repeatedly reading and reflecting on the same page, when I was pleased with the author, and thought his relation of facts and remarks interestingly connected with my enquiry: I then indulged in commenting on them, consistent with the general laws of the animal economy, and the best doctrines relating to general

pathology, and the immediate connection of the sensorium and nervous power with the vital and animal functions of the human frame. In this research I found none more consistent, none so extensive or better adapted to the experience I have had, and to the observations I have made, than those I heard, and imbibed in my early days, under the intuitive mind of a CULLEN; and I must add, that whatever new practical ideas I may have since acquired from a large and regular experience, I feel disposed to attribute their birth to the rational and fundamental principles of medicine, which that great professor so well laboured to establish, though he did not succeed in obtaining a general admission of them. I can only attribute this reserve in the minds of some to those difficulties, which stand in the way of adapting any one system to the variable opinions of mankind; and to the rage and amusing love of controversy. I should not have presumed to have said so much of my own opportunities, but with the wish and view of meriting more the attention of my reader.

6. I cannot in this place pass over in silence the memory and labours of a man, who was cotemporary with CULLEN, and who had so ingeniously employed his mind to investigate and improve the deep and dark science of medicine; whose opinions, though peculiar and very limited, have obtained much credit, and many converts and admirers, particularly on the Continent. I fear there has been too much eccentricity and narrowness in them, to lead to the desired end; namely, a more successful practice. On a simple view of them, danger to the sick even stared the well informed practitioner in the face; for his genius had led him to generalize too much the

laws, and actions, or powers, of the animal economy; and he must thereby have tempted the young practical adventurer, to combat with disease, before he could possibly have laid up a sufficient store of medical and philosophical science, to guide him through the labyrinths and difficulties of the art. I speak of the late Dr. JOHN BROWN of Edinburgh, (with whom I was there personally acquainted) a man, whose efforts in the cause of physiology and pathology can never be forgotten. The Editors of the before-mentioned Journal have with great propriety observed that “to the generalization above alluded to, he had sacrificed the experience of ages for the hypothetical dogmas of his own new practice.” I nevertheless think his new ideas on the subject, and simple arrangement of diseases into the sthenic and asthenic, have served one good purpose; namely, have disposed the minds of medical students and practitioners to seek a closer investigation and knowledge of the Cullenian system and practice of physic, and have tended no doubt to heighten in their opinions the value of both; for they both preserve their respectability and importance, and continue at this time to be taught in all our medical schools. I shall therefore find but little occasion to notice at present the Brunonian doctrine.

7. It may however be remarked, that all the observations and experiments hitherto promulgated, though many are rational, and may ultimately serve to elucidate my reasoning on the subject, will not yet tend, I fear, to any greater advantage in future to the human race, when exposed to epidemic diseases of the most malignant and rapidly fatal character, unless some certain change of practice, as well as opinion, can be establish-

ed from them, for the future conduct of the practitioner on such trying occasions; so as to do away altogether both the idea of a specific fever, as well as of contagion connected with it. The *desideratum* wanting is a clear and perfect knowledge of the affection, which I have reason to believe is not only wholly unknown, but that it is local, or topical; that the whole of the symptoms, as well as the fever, are symptomatic of a certain local affection; and that until it is considered and treated as such, it will continue to be still as fatal as it has hitherto been, whenever it should in like manner again occur. What that local or topical affection may or can be; how occasioned; and how it is to be prevented and cured, will become the main object of this undertaking to elucidate; and which I shall attempt in part to do by analogy with other known disorders, to which I think the late Gibraltar malady was, as well as the commonly called Yellow Fever of the West Indies has always been, more or less allied.

8. The recent opinions and experiments already published, and to be hereafter noticed, respecting the non-contagiousness of the disorder, will strengthen the investigation I am about to attempt; and I shall on the other hand venture to foretell, that *these very opinions*, as at present entertained by Dr. RUSH and many others, and by some very rationally supported, though not yet satisfactorily proved by any, will, I flatter myself, receive additional weight from *the particular sentiments I have adopted*, respecting the specific nature and true cause of the late pestilential disorder at Gibraltar. I may therefore, in the progress of my undertaking, have occasion to notice the said opinions more particularly: *Both* together may, I trust, ren-

der all future provision against receiving infection, such as lazarettos and quarantine, evidently nugatory and unnecessary. Still there is that wanting to my present purpose, which has never been sufficiently sought after, or carried to its full and proper extent, from the so very strong and general opinion entertained of the disorder being of a truly idiopathic febrile character, and that too of the most contagious and malignant nature:—I mean the strict attentive examination of *the body* after death, so as to have led practitioners to a complete discovery of that local morbid affection, to the existence of which I am disposed to attribute, according to its degree and extent, all the symptoms, as well as the particular fatality of such febrile epidemic complaints.

9. In the progressive treatment of this very extensive and important subject, I shall be gradually and insensibly led to those essential points, or *data*, which will then appear to have justified my present manner of introducing the particular object I have had in view by this publication. I especially beg leave to address it to the serious consideration of teachers of medicine, and all the intelligent members of the medical community, who cannot, on the principles I proceed, be led astray in a concern of this momentous nature; and who, I am sure, will with candour attend to its defects, as well as to its difficulties and importance; and when in their power further elucidate the subject for the benefit of the world. I most earnestly call upon the attention, assistance, and indulgence of those, who have already been, and of those who in future may be, engaged in continental and West Insular

practice, firmly hoping, that through the blessing of Providence, this happy country will never be afflicted with the like disorder. If the subject should prove worthy the notice of such, I trust we shall never hear again of such a mortality of the human race in this or any other part of the world, as was so recently experienced at Gibraltar, under the most trying circumstances to the country.

On the best Mode of studying Medicine.

10. The great difficulty in the practice of medicine is not only the just discrimination of diseases from one another, as to their affecting the same or different organs, but when their effects are general and when topical: for often we are equally embarrassed to decide when they are universal or general, and when local or topical. Generally speaking, universal disease affects the whole system; local disease only particular parts of it. I question however the propriety of the opinion, that apoplexy and inflammation are of the first kind; for it most frequently happens, that the system becomes universally diseased, in nervous and inflammatory affections, from local causes injuring, or disturbing, the functions of the brain and the circulation. I feel disposed to go still further, and do conceive, that a febrile state of the system, whether of the synocha, typhus, or synochus character, almost always depends upon some local affection. Therefore every febrile state of the system may lead to danger; and therefore what a modern reporter of respectability has advanced, “that no person *ought* to die of fever” (Medical and Physical Journal)

may be just, if we always could, and did, discover the local cause supporting the febrile action of the system.

11. But we live at an æra so strange and unpromising, and still so far removed from a perfect knowledge of our profession, if we are to give credit to all we read, that even the physis pulmonalis, which I may with propriety call the *endemic* or indigenous disease of our own country, is thought to be a complaint at this day so little understood, and for which other pulmonary affections are *even now* mistaken, after all that has been written on that subject, that it has been advanced in a late number of the Medical and Physical Journal, “that the patient more frequently dies from a consumption of medicine, than from a consumption of the lungs.” This is an *opprobrium medicorum*, a declared *error judicii*, without mercy. I may however add, so unenviable is a professional man’s life, that it would be happy for all, if there were no more *errors* of this kind in the general practice of fevers, than there are in the treatment in general of pulmonary disorders. I believe the latter, notwithstanding the above declaration, are better understood than the former.

12. Teachers of medicine have naturally started the question “on what plan would the study of physic be most advantageously established? Should it be on an empirical or a dogmatical plan?” If we adopt the empirical plan of practice, it is incumbent on us not only to know of many remedies applicable to the cure of particular diseases; but we ought also, and in fact we do, know, that many diseases so resemble

each other, that without the aid of theory and science to guide us, we should with difficulty distinguish them; and should not even acquire such necessary knowledge, without having waded through a great deal of bad practice to acquire it; and even after all should have it imperfectly. It is therefore important that we should, early in life, learn to discriminate well by science and application, because diseases severally require different and perhaps specific medicines to remove them. Therefore, if certain remedies are best adapted to particular diseases, we ought most studiously to acquire that knowledge, by which we have the best rules to distinguish them, that we may not unnecessarily clog the practice, and prolong the cure, (if we have the good fortune in the mean time not to do harm by the defects in our knowledge) by administering remedy after remedy in the same disease, before we hit upon those best adapted to its removal. The practice however being certainly both conjectural as well as experimental, we should be the more cautious how we refer only to our dogmatical, or confide in our empirical, mode of treatment.

13. The great professor, CULLEN, strongly inculcated a watchful attention on this head; and steadily advised his hearers not to practice medicine with a superficial and indistinct knowledge of the science; and in order to discourage the practice on an empirical system only, observed, “that reasoning and experience in medicine are truly inseparable.” “That a just knowledge of morbid bodies depends upon our correct knowledge of them in a healthful state;” and for that knowledge strongly “pointed out the necessity of having recourse to anatomy, physiology, and pathology,” as the best

ground for establishing a successful method of cure. I therefore think with CULLEN, “that theory and experiment should be connected, in order to place the practice of physic on a firm foundation.”

14. Of all the modes of education on a dogmatic plan, which have been recommended by different teachers, I most approved of CULLEN's, as I thought I saw it built upon a better selection of facts and observations, derived from anatomy and practice, than any other: and though I long ago differed from him, particularly in his supposed sympathy between the muscular fibres of the stomach, and the extreme vessels on the surface of the body, in his explanation of febrile phænomena, that is, differed with him not in the principle, but in the application of it anatomically and physiologically considered, (See my Treatise on Sympathy, p. 120 et seq.) I still found his theories in general so well supported by facts, and grounded in correct anatomy, at the same time as much simplified as the subject would admit, that I feel I have reason to attribute what I now better know of my profession practically, to the soundness of his doctrines, and to the near approach to perfection in his system of medicine, the fruits of a long life of research and study, and of a laborious application of them to observation and experience.

15. Of opinions in medicine, founded on the minutiae of anatomy, I presume I could tolerably well judge in the earliest period of my professional career, from having attended six courses of the late celebrated W. HUNTER before I went to Edinburgh; which made me feel very forcibly the observation thrown

out by Dr. CULLEN, on the use of anatomy. “Anatomy,” said he, “in its importance to the practice of medicine, may be compared to the organization of a watch, or any very complicated machine. We ought to view it in forming our judgment of diseases, as to the body’s component parts, as the mechanic refers to the perfect construction of his work, when he views it for the purpose of repair.” The leading step, therefore, to early success in the practice of medicine, is a just and proper distinction of diseases from one another. But the perfection of this knowledge can only be founded in anatomy, and gradually acquired by an attentive observation, and long practical experience; and such a co-operation must add great weight to the instructive powers of every teacher, as it, no doubt, had done to those of Dr. CULLEN.

16. I shall conclude these important observations by mentioning, as a necessary axiom in education, that minuteness in teaching must not be complained of. It is of the first consequence, in order that the young practitioner may afterwards the more readily establish for himself a just diagnosis of diseases, and a clear discrimination between universal and topical affections, as to their extent, and degree: and thereby the sooner form for himself, by his own experience, as ready and perfect a system of practice, as the difficulties of the science will permit. Let him well remember, that the symptoms of diseases are no more than so many instances of the *functiones læsæ*.

17. These most important consequences are not only annexed to the discriminating between universal and local diseases, but

also between idiopathic and symptomatic fevers. True idiopathic fever seems to me more strongly marked, or even characterized, by a greater disturbance, or increased action of the heart and circulating system, than when a febrile condition only attends symptomatically any local inflammatory affection of a vital or animal organ, or of any external part of the human frame. This will be, in due place, more distinctly pointed out : but as connected with the preceding sentiments, it has demanded this allusion at present.

18. I must in this place frankly own, that after twenty-seven years of constant experience, (2) I am more sensible *now* of the difficulties of the science, than I was when a young man; and this must apologize for my having hereafter so largely entered upon the general doctrine of febrile affections, as connected with my proposed enquiry : thinking such *data* will, as the preparatory ground, the more readily assist *some* of my readers in comprehending my views, and in the hope of rendering *them* familiar with pyrexial disorders, as I may have become myself, from an experience, I may say, of nearly thirty years.

On Life, Health, Disease, and Death.

19. Some general observations on the science of medicine, and the laws of animal life, as connected with the various deviations from a perfect state of health, may here be properly introduced, as pertaining to the object of this work. Infancy, youth, manhood, and old age, have all an instinctive interest in the progressive stages, and situations of human life, as referable to the ensuing remarks, in as far as each, for a

given but uncertain period, and in a lesser or greater degree, must experience health and disease, and ultimately death, either through casualties not always to be guarded against, injuries to the frame through misconduct, or corporeal weaknesses and imperfections in the animal machine, from its primordial conformation. The above interest will not be *equal*, but of course bear an inverse proportion to the prospect and probability, that each individual may have of a shorter or a longer existence. Sir JOHN SINCLAIR, Bart. in his intended publication on health and longevity, proposes, that one part of it should consist of regulations for the health of the community; and he has already endeavoured to impress his readers with the importance of his views, by the following allusion to the late mortality at Gibraltar. “The police of the Public Health is a most important branch of our proposed enquiry; and the events, which have recently happened in Spain, and at Gibraltar, have given it an additional interest.” He then enumerates the general heads, under which it may be treated. (Medical and Physical Journal, No. 76.)

20. Mr. EDMONDSON, in his account of an epidemic ophthalmia, which he traced from Egypt to Gibraltar, observes, that “Modern physiology had done much towards explaining the laws of animal life:” and that “the changes which take place in the system in the different states of health and disease, are more accurately known, and better explained, now than formerly.” Here it may with some propriety be remarked, that there are still so many parts, or functions, of the animal œconomy in a state of health, neither fully investigated, nor satisfactorily explained, that the difficulties in the way of esta-

blishing, either regulations for the preservation of health, or systems for the prevention and cure of diseases, may be readily admitted, and easily accounted for, particularly those connected with atmospheric changes. It would aid much the above philanthropic views of Sir JOHN SINCLAIR, if we could better understand the complex physiology and pathology of the skin, as an important organ of the human body, which, as acting in conjunction with the general functions of life, is yet, though anatomically well explained, but imperfectly as well as insufficiently investigated, notwithstanding the recent experiments and labours of the most scientific anatomists, and chemical philosophers. Let us however hope, that their prosecution of the subject will throw further light on this intricate part of the animal economy.

21. The science of medicine will be found not so difficult to attain, as the just and proper application of it to practice. After the many centuries of observations, which have been handed down to us from the time of Hippocrates to the present day, as a guide to the young practitioner, it is painful to observe, that the profession is still in an open sea, sailing in every direction to discover the tract most successfully leading them to the temple of Hygeia; and to the prevention and cure of diseases. In this sea of discovery on the present occasion, I acknowledge myself to have been much tossed about. General physiology and pathology have been my great guides. I have certainly laboured under the disadvantage of wanting the main pilot to a sure and safe anchorage—to wit, positive experience *at the scenes* of raging pestilence, to assist and confirm

my own practical reflections on the subject (1). I have therefore done the best I could, without having witnessed what the medical practitioners at Gibraltar have had the benefit of, if they should ever publish on the subject. Nevertheless, from some proper sources I have endeavoured to obtain the facts and observations resulting from their experience, which, I trust, will serve the place of the pilot of this my little adventurous bark.

22. I shall first attempt to express my opinion in what perfect health consists. Animal existence depends upon a certain immaterial part, THE SOUL OR MIND, joined to a material part, THE BODY. The nervous and vascular organization of the latter is importantly dependent on a just balance in its various functions, as connected with the former. A deviation from this due balance produces more or less disturbance in the mind or soul; and lays the foundation of the different degrees and kinds of sufferings and diseases, with which the human body is, through life, afflicted. When we enjoy a perfect state of health, we have none of those corporeal sensations, indicating to us, that *we* exist in such a material form. We are then all *soul*; and perform all the voluntary actions of this life, as connected both with mind and body, with the consciousness only of the connection, but without feeling corporeally the union. When we do feel it, we are then instinctively led to consider, as rational creatures, what such feelings are occasioned by: if natural we indulge them, when proper: if not proper to be indulged, we are morally in fault when we do; if morbid, we are then feelingly disposed to seek relief in the science and art of

medicine, in order to restore that disturbed balance in the corporeal functions and sensations, with which our soul in the state of perfect health is not sensible of being united. Perfect human health is, therefore, the enjoyment of animal life, and our reason, without the smallest sense of pain in the performance of the various corporeal functions connected with them; no otherwise than is the natural action of the organs, as suited to their several purposes. GARTH, in his Poem, "The Dispensary," when describing *health*, says,

"She seem'd a Cherub most divinely bright;
More soft than air; more gay than morning light."

23. I shall next add the opinion, which the great CULLEN entertained respecting the immediate source of animal life, as being, in a great degree, illustrative of the communion, *ab origine*, between the soul and body, and ultimately connected with the inquiry I have in view. In the first place, he supposed "the nervous system to be the origin of the soul, and therefore of animal life: and to be of such a nature, as to admit of an *aura*, a *lymph*, or fluid, for the communication of sense and motion from one part to another: but that this fluid was, at different times, in different conditions of fitness for this communication, which different states or conditions, he named the greater or lesser excitements; and which excitement he supposed to be that condition, when in the perfect state, which is the foundation of life and health." He further supposed, "that according to the degree of increase, or diminution of this excitement, would be the particular form of fever:" And as all the separate and component parts of the

nervous system have a communication with each other in their common origin, the brain, so he supposed, “that we might discover the variable changes of excitement in the brain, and the reason for its affecting the other component parts of the nervous system.”

24. He further observed, “that as the principal parts of the animal functions were dependent upon the excitement of this system, so the preservation of life consisted in that excitement; and that a total collapse of the nervous power, after it has been once fully excited, was the cause of death.” Thus, a due and proper excitement constitutes life and health; a morbid increase or diminution of it occasions disease; and the total collapse or extinction of it is the state of death. He conceived it out of his power to say “what at first begun the excitement,” but he thought it evident “that, whatever the power it possesses in consequence of an original impression, life cannot be long supported without it.” Thus we see, that although the soul and body be distinct, yet their motions are intimately connected during life.

25. Again, “It is well known,” said the professor, “that there are various external powers, which increase and diminish the above excitement; and that many of the functions of the corporeal organs are requisite to keep it in proper condition.” I therefore think the causes of death may be referred to one or other of the two following heads: viz. 1st. Either to such as act immediately upon the nervous system, or the brain, so as to disturb, or entirely destroy, the excitement. Or 2dly, to such as interrupt or destroy the circulation of the blood, or the

other functions necessary for the support of the nervous excitement. The first of these Dr. CULLEN considered as direct causes, the second as indirect causes, of death.

26. Dr. CULLEN concluded this brief explanation of his opinion respecting life and health, by observing “that death was not always the immediate consequence of a cessation of the circulation,” for that a frog would live an hour or two after its heart was cut out: but that in time, from the cessation of the circulation, it would have destroyed the animal: only it would not then have been considered as the direct cause of death. Whereas, if the brain was bruised, or much shook, or the circulation through it in any way obstructed, or deranged, immediate death might probably be the consequence. Therefore the direct causes must act thereon, to produce a speedy dissolution. He enumerated, as among the principal direct causes, cold; the sedative passions, such as fear, &c. convulsions, such as the epileptic, &c. and sometimes the excess of joy; and electricity. Each of these have proved fatal by the sudden and great excess of excitement, caused by them, being immediately followed by an excess of collapse; all which, when they do not immediately, or very quickly, destroy, may be the cause of subsequent fever. Notwithstanding all this, Dr. CULLEN did not consider compression of the brain, as the cause of death in febrile pestilential disorders.

27. I ought not to omit in this place to briefly mention the fundamental principle of the Brunonian doctrine, with a few remarks on it, leaving my reader to apportion the weight and

consequence of either, as connected with the human economy, in the general scale of medical science and improvement. Health, disease and death, must have an immediate interest in the said principle, which is, “that a determined portion of excitability is assigned to every individual at the commencement of his existence.” And it has been further advanced by Dr. BROWN, that “the common effects produced by the exciting powers are sense, motion, mental action, and the passions; which effects being one and the same, it must therefore be granted, that the operation of *all the powers* is also one and the same.” “The effect of the exciting powers, acting upon the excitability, is excitement.” These opinions are however very well combated by the Editors of the Edinburgh Medical Journal, who have pointed it out as unfortunate for the Brunonian system, “that the author has made no provision in it for the recovery of exhausted excitability.” This charge has therefore been brought against its consistency; and in order to make the imperfections of this system more fully appear, they have observed, “that some passions of the mind are highly exciting, and others are evidently depressing; but that, as a direct sedative had no place in the system of BROWN, the passions having this effect had been sacrificed by him.” He seems to have advanced, “that the depressing passions have no real or positive existence, and have only been considered by him as negations merely of the exciting passions.” “Fear and grief,” said he, “are only diminutions, or lower degrees of confidence and joy.” As more immediately pointing to our subject, it may be proper also to observe, that BROWN, consistent with his principle, considered, “that contagions were stimulant in their mode of operation:”

This opinion has also been very fully canvassed by the Editors of the Journal, and I think very properly; they having questioned, among other things, that “the great *debilitating energy*, observable in *certain contagions*, does not more prove a diversity of action in them, than it does in the case of an equal or greater degree of debility arising from cold.” But, in order fully to comprehend the reasoning of the Editors on the subject, I must beg leave to refer my reader to the third number of the Journal.

28. The Editors have further admitted, as an improvement of the present age, that in the room of the metaphysical, mechanical, and chemical theories of the last and preceding centuries, the physiologists and pathologists of the present day, “seem more prudently disposed to regard the powers of muscular motion and of sensation, as ultimate facts in physiology,” on which I trust, through future observation and experience, will be founded, a more successful theory and practice, “by a *just* investigation of their laws and conditions of action.” Spight however of this acknowledged improvement, when drawing their strictures on the speculative dogmas of the Brunonian system to a conclusion, they say “the doctrine of excitement, the laws of excitability, and the theory of the operation of the exciting powers, as taught by BROWN, have appeared inadequate to the explanation of the phenomena of life, of the actions of the natural and hurtful powers, and of the operation of remedies:” and they finish their criticism of BROWN’S works at large with observing, that “the doctrine of excitement, even if the laws of excitability were fully ascertained, would still be imperfect as

a theory of life, which must also include the doctrines of the chemico-material processes, constantly operating in the systems of organized beings.”

P. S. In treating my subject in the several sections constituting the first part of my work, I shall occasionally introduce my own particular *doubts* and *opinions*; but I beg that *they* may be generally received as founded upon those principles and sentiments, which formed and supported the Cullenian doctrine, lamenting, that the present state of medical science, notwithstanding its improvements, cannot further assist me. It should, however, be observed, that as the science of life is daily acquiring new votaries, all aiming at the promotion of its perfection, much more may be discovered, which will add to our present means of *prolonging life*, of *preserving health*, and of *curing and alleviating disease*. The deep and extensive views and researches of Sir JOHN SINCLAIR, Bart. before alluded to (19) must greatly tend to an amelioration of the comforts of society, and to an alleviation in general of the variegated miseries of mankind. It is on this ground to be hoped, that, in process of time, a complete illucidation of the *arcana* of human existence may insensibly lead the attentive mind of the medical practitioner to a clear investigation of those hitherto dark and dangerous afflictions, which are daily terminating the life of man, at all ages, and even in the most civilized parts of the globe. That the illustration I have in view may be the more readily comprehended by the younger members of the medical community, has been my motive for commencing with some general remarks on the MODERN STATE OF THEORY, AND PRACTICE IN FEVERS.

OBSERVATIONS, &c.

PART I.

ON GENERAL PRINCIPLES CONNECTED WITH THE PATHOLOGY OF PYREXIAL DISEASES.

SECT. I.

ON THE CAUSES, SYMPTOMS, AND VARIETIES OF FEVER.

29. MANKIND has most probably been doomed by original fate to be exposed, at some time or other of their lives, to fevers arising from specific miasmata, and they severally seem to be particularly connected with the different organs or functions of the system. There are some grounds therefore for suspecting, that all the several vital and animal organs of the human frame must have been originally intended to be made subject to the effects of some specific atmospheric action, or contagion, once in the progress of life. Such are the small-pox, measles, chicken-pock, chin-cough, &c. which, as arising from specific contagions, connected with the atmosphere, must, I presume, have existed from the beginning of the world. It may however have been ordained at the creation of things here, if we can be allowed to freely reason *a priori*, that certain specific human contagions should be inherent in the natural constitution of man, and should afterwards become indirectly epidemical from one part of the world to another, through the necessary communication of the animal economy with the atmospheric air, on every part of the habitable globe. The *arcana* of human existence, and of human sufferance,

will justify any conjectures on this intricate subject; for the doctrines of contagion are still involved in much obscurity, through our still imperfect acquaintance with the theory of life, as just alluded to (28). How far the febris scarlatina, as an exanthematous disease, will come under this consideration, may be made a question. Though arising apparently from a specific contagion, and strongly epidemic at times, and supposed also to affect us but once in our lives, it is not so common, as the other disorders just mentioned. It has been unfortunately sometimes not distinguished from the measles: I have known the latter to have been mistaken for the former, though an angina particularly characterises the febris scarlatina. CULLEN had observed this species several times regularly epidemic in Scotland. The cynanche maligna has been of late considered as a modification or variety of it.

30. I feel it very difficult to fix the limits of general observations, and am unwilling to compress the subject too much, because I think the young practitioner will find his account in having the principal leading circumstances of fever pointed out to him, preparatory to my immediately entering upon the object of my enquiry, as such will open and prepare the mind previous to my investigating the particular nature of the Gibraltar disease. I cannot therefore do better than give, as briefly as possible, the general outline of fever, with all its collateral points, which seemed to me so satisfactorily admitted, as evidence of the Cullenian doctrine. Here allow me to introduce the words of Dr. BLANE, in his "Observations on the Diseases of Seamen," when he alludes to the just application of Dr. CULLEN's doctrine, respecting the re-action of the system, as they so much accord with my sentiments of his departed worth and talents. "I am happy in the opportunity," says Dr. B. "of acknowledging my obligations to this worthy professor, to whom the medical world in general is so much indebted, as well for the rational views of the animal economy, which he teaches, as for that spirit of study and enquiry, which he infuses into the minds of his pupils." I will therefore

proceed, confident of my reader's approbation of the plan I adopt, and of the preceding motives.

31. I shall first observe, that of all the morbid affections, to which our natural or material existence is in this world exposed, none is of greater extent and moment, or more often fatally experienced by mankind, than the class of pyrexial diseases, and particularly the *febres* of CULLEN's nosology. They spare neither age, nor sex, and have ever been at times most mortally prevalent in every quarter of the globe. GARTH, in the poem before alluded to, has very emphatically described the universality of those maladies in the following lines, in fictitious allusion to their dwelling-place; and gives them pre-eminence of extent and mischief over all the other disorders.

“ Within its dreadful jaws those furies wait,
 “ Which execute the harsh decrees of fate.
 “ FEBRIS is first: the *bag* relentless hears
 “ The virgin's sighs; and sees the infant's tears:
 “ In her parch'd eye-balls fiery *meteors* reign,
 “ And restless ferments revel in each vein.”

CANTON VI.

32. The paroxysm of a fever consists of three states, and may prove diurnal, or continued, according to the operation of the remote cause. 1st. The state of nervous debility. 2d. The state of spasm or constriction. 3d. The state of increased action of the heart and arteries. The intermissions, exacerbations, and remissions, are differently marked, and of more or less extent in different fevers.

33. Fever commences with (more or less of) horror, tremor, and rigor: they are marks of the loss of tone, or balance between the nervous and muscular powers, and the less they are felt, the greater is the subsequent fever. Why should debility be supposed the first operating cause of fever? The opinion can only be founded on the supposition of the re-action it excites, in order to restore the lost or disturbed balance in

the circulation. The remote causes of fever producing this debility are ever worthy of consideration. They may with propriety be considered as poisons, which weaken the nervous power, and energy of the brain (23 et seq.) Of the principal of these I shall speak more at large hereafter.

34. There is one organ of the body particularly affected at the attack of fever, from its established sympathy and connection with the whole system, to wit, the stomach: so that the nausea and vomiting at the commencement of fever, are the effects of sympathy with either the general debility of the system, or with some local affection; and do not ordinarily arise from foulness of the stomach.

35. The state of the bile has been supposed by some to be the most probable cause of fever. Its presence however, as we generally see it, is certainly only symptomatic. On this account, emetics may often be dispensed with, particularly after the commencement, or complete formation, of the fever: and when the nausea and vomiting continue beyond the commencement of the fever, lasting for several days, perhaps a week, they are the symptoms of sympathy, and should point out to us, from the observations I have myself made in practice, a suspicion of some local inflammatory affection existing in some one part or other of the body.

36. Though horror, tremor, and rigor, are generally to be considered as the effects from cold, by producing debility at the attack of fever, yet such effects will often be alone produced by the passions of the mind, as fear, &c. which, spight of the Brunonian doctrine, are all “affections equally positive and independent:” and it may be proper to observe, that tremor, while it arises from debility, will bear a proportion to the subsequent spasm and power of re-action. Tremor, in fact, has been considered as the beginning of spasm. It is of a paralytic nature: and when the horror, tremor, and rigor do not take place, it argues a want of proper exertion in the system to overcome the debility, and consequent derangement in the circulation (22).

37. Further, it is proper to observe, that the affections of the stomach, even at the commencement of some pyrexial disorders, are not to be wholly attributed to the occurrence of debility and spasm of the extreme vessels, agreeable to the Cullenian doctrine ; but may be viewed as properly establishing a just ground for seeking out a local affection, with which the stomach sympathises soon after it has taken place.

38. There cannot be a better proof of the probability of vomiting in fevers proceeding from either a general nervous cause, or a local affection, through an established sympathy between the stomach and the rest of the system, than what so frequently happens after blood-letting : namely, the *deliquium animi* and sickness. The vomiting also, which is so commonly produced by a blow on the head, is a further proof of a mutual sympathy and connection ; and when there are just grounds to apprehend any local affection exciting it, we should be cautious in administering emetics. For it should be remembered, that severe vomiting is at all times liable to prove hurtful, under any local congestion (35).

39. Fever has, by many writers, been considered to arise from the state of the bile ; and SENAC, with others, as just noticed, thought it the most probable cause of fever. Yet he overturned his own doctrine, by saying, “ that the greatest redundancy of bile might not only exist in the system, but even be thrown out of it without producing fever.” There is certainly a proof of this in icterical disorders, they being generally unattended with fever. I consider the liver as most liable to chronic disease, though occasionally to inflammation. CULLEN even admitted, that though the bile is not necessarily the cause, yet that in autumnal fevers, a redundancy of it might occur, and aggravate the disorder.

40. A redundancy of bile is most apt to produce intestinal complaints : and it may with justness be observed, that patients prone to be afflicted with bilious disorders, such as the autumnal dysenteries, are least liable to fevers. It may therefore be advanced, that the cause of fevers, and that of dysenteries,

must be very different. I am of opinion, that they both may arise from membranous inflammation of the primæ viæ, occasioned by the acrimony of the bile: I mean, both the autumnal fevers, and the dysenteries, the febrile symptoms then proving symptomatic only of the local affection.

41. A perfect state of health depends on a just balance between the heart and all parts of the circulation, (22); particularly between the external and internal extreme vessels, or the extreme parts of it: while fever, as a deviation from a state of health, is characterized by, and most probably arises from, some derangement, or alteration in it. Thus pyrexial diseases are distinguished from other diseases by the frequency of the pulse, marking an increased action in the heart and arterious system (266).

42. The febres of CULLEN's Synopsis Nosologiæ Methodicæ are characterized by the pyrexial symptoms being independent of, and unaccompanied by, any local or topical affection (267). The other febrile disorders of CULLEN's class pyrexia, as enumerated in the four subsequent orders of the Synopsis (270) are distinguished from idiopathic fever by their local and topical affections, and from each other by the difference of those topical affections, namely, as either being phlegmasial, exanthematous, profluvial, or hæmorrhagic.

43. The length or duration of fever depends upon the natural nervous energy of the system continuing in a partial state of debility, or collapse, independent of the peculiarity of the vascular temperament, or of the mode of attack, or of the proximate cause; and may be protracted, even for a length of time, after any local affection is subdued.

44. Proper pyrexia depends upon congestion in the arterial system; and it will equally explain the principal phænomena of inflammation. And it is, moreover, this congestion, which produces that irritation, or constriction in the arterial system, which gives rise to the diathesis phlogistica, or inflammatory diathesis. The truly continued fevers are always more or less accompanied with the inflammatory diathesis.

45. The accurate Dr. CULLEN has observed “that pestilential fevers are sometimes suddenly fatal, from excess of debility at the attack; and sometimes, but rarely, the same excess has destroyed on the attack of an intermittent.” Whether a local injury is the immediate cause of such sudden death, on both these occasions, I may hereafter attempt to explain. CULLEN even conceived, that *sudden* death during the hot fit, took place on the same principle.

46. I shall describe shortly the character of an inflammatory fever, as to the predisposition to it, its rise, attack, and progress, as such will be applicable hereafter to the purpose of my work. This may be considered the synocha both of SAUVAGES and CULLEN.

This fever is most common in cold seasons and climates, and generally attacks those of a robust and sanguine constitution: and such as are liable to hæmorrhage. It attacks suddenly, and with little languor, and does not proceed from contagion. There is little of horror and nausea, and the cold fit is very short. The face soon becomes florid and turgid, and is accompanied with so great an increase in the action of the heart and arteries, as generally to cause some degree of deliquium. The hot fit soon succeeds, and is attended with head-ach and throbbing of the temples, pain of the back, and the joints of the extremities, and a frequent, though not difficult, respiration. The pulse is full and quick; the thirst considerable; and the urine is high coloured and without sediment. Such a fever is of uncertain duration, seldom, however, exceeding a week, varying, according to its termination by hæmorrhage, sweat, or urine. The exacerbations and remissions in this kind of fever are very obscure.

47. It will also be found necessary to the purpose of my investigation to contrast the preceding account of an inflammatory fever with the ordinary description of the attack and progress of the nervous fever, or typhus: as follows.

The typhus is said to be the product of warm climates and seasons, and to arise from contagion. Those persons

much weakened by cold, or any other debilitating cause, are most liable to its attack. It comes on slowly, and is gradually attended with loss of appetite, anxiety, languor, and lassitude. At the approach of night, a sense of cold and chilliness, with slight horror, takes place. The patient becomes hot in bed, and the sleep is confused. The symptoms nearly go off in the morning, but come on again at night, and the hot fit becomes stronger. This kind of fever gradually increases for several days. The heat is moderate in the day-time, with a pulse frequent, but not hard. The animal functions become much impaired. There is a despondency of mind, an anxiety, nausea, vomiting, and total loss of appetite. The sleep becomes variably disturbed, and delirium soon succeeds. The heat of the body becomes unequal: the lower extremities are generally more or less cold: the face grows pale: the belly becomes irregular, and the urine limpid and clear, without sediment. The exacerbations and remissions become obscure, and, the fever is protracted two or three weeks, or more, without any sensible crisis. The general symptoms are such as point out a debility of the sensorium, and can only be explained by a prevailing debility in, or *some derangement* of, the primary functions of the brain.

48. Fortunate would it be for mankind, and most gratifying to the profession, if fevers, admitting that there was no local cause for them, could always be so characteristically marked and distinguished; for the treatment of them would then be plain, and eventually successful. But the oldest practitioner must allow, that their phænomena alter so much, and are so variously mixt and combined, from a difference either in the operation of the remote causes, or in the predisposition and temperament, or in being connected with some hidden local affection not suspected, that the most judicious administration of medicine will fail, when ultimate danger had been but little apprehended. From this variability of character, and distinct from the supposition of local affection, nosologists have established a third genus of fever, the synochus,

which, ordinarily speaking, first puts on the appearance of inflammatory fever, and soon after acquires the low nervous type.

49. Further, fevers have been supposed, by some writers, to have their origin principally in the admission, or absorption, of contagious miasmata. When such is the case, a ferment is then supposed to be produced in the circulating fluids, which communicates contagion to the whole system, the multiplication of which produces that particular fever, called the putrid. This may be the sequela or termination of the others; and as there are certain diagnostic signs of a putrid fever, it may be proper, for the main object of this work, to point them out. However, I must observe, that I think the sound and natural crisis of the blood is not so much changed, as teachers generally have supposed. The phenomena may admit of a different explanation.

50. In the putrid fever, the blood drawn from the veins has been observed not to coagulate, and the serum of it is said to be reddish,—that blood will rush out, without any external cause, from the gums, nose, ears, eyes, and lungs, without any pain. Bloody stools will occur without any diarrhæa; and the urine will be bloody without nephritic complaints. It has even been advanced, that blood has oozed through the pores of the skin. But that particular marbly appearance of the skin, formed as it were from an infinite number of purple spots, has been said to be occasioned by blood extravasated into the rete mucosum, and seen through the cuticle. These spots or appearances have been distinguished by the term *petechiæ*: But, from what I have witnessed, the true petechial spots do not put on that marbled or mottled appearance of the skin, which is so frequently seen without fever; this appearance may be very often noticed as a strong mark of health, and certainly depends at the time upon a peculiar condition of its vascular structure.

51. The highly putrescent state of fever is further said to

be accompanied with a foetid breath, foetid stools and urine, and an extremely cadaverous smell of the whole body. The putrid fever may be considered as a species of the synochus, and becomes more or less malignant, according to the preceding degree or prevalence of the inflammatory or nervous symptoms. Even this putrescent state will cease increasing after death, unless the heat of the atmosphere is excessive: which proves, I think, that contagious miasmata cannot be the sole support of putrescency, as a ferment.

52. It however has been received as a general observation, that whatever the symptoms of putrescency may be, the putridity most probably arises, (if general opinion can be depended on) from contagion received into the system by means of a particular effluvium, which may at the same time act both as a sedative and a stimulant, and thus first produce the synochus, and then the inflammation: and CULLEN used to assert very positively, “that all fevers arising from human effluvia, would be attended with more or less of the typhoid character.” But he found in his day, and it is I believe much the same at present, that the difference of marsh and animal effluvia was, and still is, much disputed: and if he had been living now, would himself have admitted, that the doctrine of contagion is not carried to the extent it was formerly: though I believe the alarm of it is still more frequently given, than it should be.

53. Putrefactive vapours are not alone detrimental to the human body. They require more or less of the operation of heat and moisture, to render them deleterious: and therefore such fevers are not often met with in cold seasons and climates; or if ever such prey upon the human frame, in such seasons, it must be, when the excess of cold may have considerably debilitated the body: and too much of the inflammatory diathesis has prevailed.

54. There is so intimate a connection between inflammatory fever, and the phlegmasiæ or local inflammatory affections, that it is difficult, from the symptoms at the commencement, to determine which has been the primary affection, or disease;

and therefore it will require some study and reflection, with a minute attention to the symptoms, to find out which has been the idiopathic complaint. On a happy discrimination in this respect depends the success of our advice and treatment. It may be further observed, that fever is most probably idiopathic, when it continues after the disappearance or cessation of the inflammation, or local inflammatory affection. The exanthematous affections connected with fever generally depend upon specific contagion, such as the small-pox, &c. When active hæmorrhages occur in fever, they are to be considered as symptomatic, or consequential to them. Let not partial hæmorrhages lead the young practitioner astray, by his supposing them to have any connection with a putrid disposition of the habit. This may be easily apprehended in tropical climates under the plethoric state of the system (50).

55. As I believe nosological science is not so much cultivated in the schools of medicine as formerly, at least has undergone no improvement or particular change of late years, it may be sufficient to my present purpose to say, that the pyrexia and the febres of SAUVAGES are synonymous terms. LINNÆUS formed no distinct class of pyrexia: But his class febriles, and SAUVAGES's febres, are both the same with CULLEN's pyrexia. VOGEL's system of fevers came nearest to the class of pyrexia laid down by Dr. CULLEN.

56. Those, who are watchful of the progress of fever, will often observe, that diurnal habits are much connected with febrile diseases, and to them the daily exacerbations and remissions in fever may in part be attributed. These are on some occasions quite obscured; and are probably more particularly so, when the fever is symptomatic of some local affection. But the greater proportion of fevers have certainly been attributed to atmospheric changes, and other aerial causes. Dr. RUSH has ventured to almost assert, "that we are *compelled* to resort to certain noxious qualities of the atmosphere, as the exclusive causes of the prevalence not only of the yellow

fever, but (with a few exceptions) of all other epidemic fevers. Yet, disposed as he now is to combat the doctrine of contagion, (4) and to attribute the spreading of the yellow fever to the exhalations from putrid matters, which are diffused in the air, he has candidly acknowledged, “that we are yet ignorant of the precise nature of those qualities in the air, which produce epidemics.” Unfortunately this frank confession is too well founded, and justifies the intention of the present undertaking.

SECT. II.

ON CONTAGION, COLD, AND FEAR, AS CONNECTED WITH FEVERS.

57. It is proper to point out the following distinctions in the febres, or idiopathic fevers, as marking the different species or kinds of them. They have been considered either as epidemic, endemic, contagious, or sporadic; but the distinction is generally lost, or confounded in practice, with the general term of the typhoid or malignant fever. The truly inflammatory fever has been supposed rare, and not of the contagious or malignant nature: but is not so rare as generally supposed.

The terms above-mentioned are thus applied.—Epidemic and endemic, or indigenous, pertain to the atmosphere; contagious and sporadic, to the human body. But contagion is sometimes spoken of as an epidemic vapour arising from the human body: so that I think it may fairly be concluded, that contagion and miasma must be sometimes complicated, or have degenerated into one another: and I believe that disease, which is truly endemic in tropical climates, from the atmospheric state, is not properly of miasmatic origin.

58. As epidemic vapours are not natural to the atmosphere, they have been called miasmata, or *inquinamenta aeris*, that is, not as the proper component parts of it, but as an acquired

ordure or defilement of it. Vapours from diseased bodies constitute contagion. But, when they are (*originally*) resident in the air, and have arisen from animal or vegetable matter, they are then called miasmata. Miasmata may very naturally be disposed to be various, but that with which we are best acquainted, is the miasma, which arises from marshy grounds, and places liable to inundations, and is produced always by the concurrence of heat and moisture.

59. Heat will not produce this miasma, provided the earth be at the same time dry: nevertheless, epidemic disorders have been supposed to rage in climates under such circumstances. Neither will moisture alone produce it; as it is never produced from pure lakes and seas. This is sufficiently proved by the annual effects of the flowing of the Nile. However, that vapour, which is generated from filth, and confined heated air in gaols, hospitals, and all partially ventilated places, partakes, I should suppose, of the nature of contagion, rather than of miasma.

60. In general, the miasma arising from marshy grounds is looked upon as occasioning fevers of the intermittent kind, and probably the autumnal tertians in all countries, which may often be considered as tertian epidemics. These may, by a too heating regimen in a hot climate, become the bilious remittent fever, and terminate fatally. When marsh miasma produces the intermittent, there is reason to suppose that it has its operation through the nervous system; and when the autumnal tertian has been converted into the bilious remittent fever by a too heating regimen, I imagine an enteritis, or the inflammatory state of the primæ viæ, has been occasioned, accompanied by a fever bearing the character of a synochus (48).

61. As contagion is so much the theme of public apprehension, I shall add some further opinions respecting its general character, and application to disease; but the subject is too diffuse to introduce every thing, which has been said upon the subject. The origin of epidemic and endemic disorders has been supposed to arise from the atmosphere, by the

inhalation of a vapour, which has been called miasma. This miasma on some occasions may have been converted into contagion, and may have also undergone some further alteration, by its communication with the human body. Contagions are not however so various, as has been generally imagined. They commonly act in producing either exanthemata or profluvia, diseases of a specific local character, such as the small-pox, measles, chin-cough, dysentery, &c. and from their known property in always producing the same disorder, have been deemed truly specific. The specific contagiousness of dysentery may however very reasonably be called into question. But febrile atmospheric miasmata being supposed capable of change by communication with the human body, so as to become contagion, cannot be considered as specific, unless admitted to vary in their effects from difference in human temperaments.

62. But I believe there is reason to apprehend, that some fevers from atmospheric causes, particularly those on the Continent, and fevers from human effluvia, essentially differ, so as not to be mutually epidemic or contagious, on the same principle, being not of the same genuine character: And, in regard to those fevers, arising either from true marsh atmospheric miasma, or the human effluvia, it may admit of a question, whether their difference of character does not as likely arise from a difference in the predisposition and temperament of those infected, as from any essential difference in their specific qualities, as infections.

63. Though the contagion of the small-pox has unquestionably a specific origin (29) yet it even produces very different degrees of disease, which must either arise wholly from the difference of excitability, or temperament, or be, in some mild epidemic seasons, more pure from communion with any other atmospheric miasma. Therefore the great variety of epidemic fevers, as recorded by some authors, must surely be founded in difference of constitution, and not in any essential

difference in the specific qualities of either atmospheric or human effluvia.

64. It has been supposed that the origin of epidemic fevers may be traced from one species of miasma only, namely, the marsh effluvia, because no other than this has been known, or from local circumstances discovered: but I even think the discovery of this is too often fanciful and imaginary. It will not however account for the intermittent and remittent fevers, and other febrile affections of the low nervous kind, which are so frequently met with in this metropolis, and other large cities. It is therefore fair to conclude, that all contagions, which are not of a specific nature, arise from one common source, though difficult to be traced, and that they are probably uniform in their effects, from some particular property acquired by heat, and their communion with the human body; but that the variety of fevers arising from such source, depends on the habits of life, and peculiarity of constitution, of those infected.

65. It would be happy for mankind, if we could reduce the system of epidemic fevers to a more limited and defined compass: But, we must be cautious how we proceed in generalizing and simplifying the nature and species of diseases arising from contagion. Dr. CULLEN thought, that the genuine contagious human effluvia adhered very tenaciously to the walls of houses, ships, furniture, and our garments, and that it therefore might affect people at a very great distance from the place, or time of its first operation: (52) Yet, he observed, that this "species of human effluvia generally acted more powerfully as a miasma than a contagion." He particularly observed, "that medical men, though often exposed to such contagion, were seldom affected by it." This he attributed in a great measure to the force of habit; but gave this cautionary advice, "that if they would not sit on the bed of the patient, they would stand a greater chance of avoiding it;" for he thought it adhered more strongly to the bed cloaths, than even to the human body. If I should however succeed in my investigation of the Gibraltar disease, it will very much alter the opi-

nions so generally entertained of contagion, and contagious fevers, both as regarding the life of man, and the commerce of all countries : and in the end fully establish the doctrine of non-contagion (4, 8).

66. As the operation of cold on the human body is so generally admitted to be a cause of fever, a few remarks on it must be pertinent to our subject, and future enquiry. When cold alone affects the body in a sound state, it produces pyrexial disease, that is, a disease of the febrile or inflammatory kind ; though excessive cold, by occasioning debility, may lay the foundation of a low nervous fever. Its ordinary operation is the production of the slightest or least dangerous fever, such as the synocha : and of catarrhal and inflammatory affections ; such as rheumatism, &c. The most common consequence of its occasioning a suppressed perspiration, is a revulsion to the fauces, and the bronchial glands ; and though cold is considered as the common cause of fevers in this country, and sometimes of very dangerous ones, it is however suspected not to be productive of such, or of any epidemic fever, simply through debility and spasm succeeding, unless the more frequent causes, contagion or miasma, have been previously taken into the body. In however the epidemic seasons of tropical climates, I think fevers must occasionally occur in the midst of universal pestilence, (such as happened at Gibraltar) from the operation of cold, on the principles I shall attempt to establish, particularly when strongly marked by the inflammatory diathesis.

67. The operation of cold may be questioned, as to the production of the gaol, hospital, ship, and pauper fevers of this climate : though the poor and lower orders of society are most liable to a kind of contagious-malignant-endemic in the metropolis, and other large cities, (connected certainly with human effluvia in a limited degree) because they are most exposed to cold, and every other particular cause, which may debilitate them. This has led so many to consider cold as the common, or sole cause of fevers in this country, from its fre-

quent connection, as a remote cause, with some of an epidemic character. But the predisposition to such fevers is caused by excesses of every kind; and when these occur in conjunction with grief, or great evacuations, they occasion the severest kind of fever: hence the rarity of the synocha of CULLEN, or true inflammatory fever.

68. It is essentially necessary to my intended enquiry, that I should set down some remarks on the operation of fear, as a remote cause of fever. It acts very powerfully on the system, and is singular in its effects. It evidently weakens the whole frame, and strikingly deranges the balance of circulation between the external and internal extreme vessels, by its nervous operation on the heart itself (41). Hence arises a sudden paleness of the whole surface of the body; and a proportionate fulness and congestion of blood in the larger vessels ensue: and this has sometimes occurred to so great a degree, as to occasion immediate death. When by a lesser degree of its operation it fails of so suddenly destroying, it affects the system as a sedative, which condition is sooner or later succeeded by an increased action of the heart and arteries. This is the ordinary acceptation of fear as a remote cause of fever.

69. The observations of the great CULLEN on this interesting subject were as follows. "Fear has," said he, "evidently the power of diminishing the energy of the system, and thereby of producing that debility and spasm, which are the foundation of fever: and according to the extent of its sedative power, it may prove the cause of fever of any degree of violence or duration:" But whether it actually does prove such, he would not positively affirm. But I believe it to be very certain, that it greatly predisposes the body to the infection, or influence of the supposed miasma, and thus may become, as the exciting cause, the foundation of the most dangerous fevers. Even GAUBIUS, as CULLEN observed, took notice of this in his pathology. It also equally predis-

poses the body to the operation of other causes, producing, according to the nature of the cause, a particular kind of fever.

70. Dr. CULLEN further observed “that fear afflicts us the more deeply according to the interest the object of our fear bears in our affections. It therefore operates not on particular constitutions, but according to the disposition of the mind.” In this way, the Professor accounted for epidemics seizing families, and intimate acquaintances, from the particular concern and apprehension excited among them: And, on the same principle, such an epidemic fever would spread on board of ship, if an alarm was given; for, according to the new opinion on contagion, which Dr. RUSH has recently conceived (75), infection would not spread at sea solely from a person who sickens and dies there, after having brought the seeds of it in his own body from a sickly shore, for these reasons among others, namely, “that the crew would not be under the morbid operation of fear, or fatigue.” (See Medical Repository, vol. VI.)

71. The operation of such a depressing cause as fear cannot be resisted. Its effects have nothing to do with constitution; and only secondarily with an epidemic, or pestilential atmosphere. But the vigour or power, and courage of the system, are such at times, as to resist the operation of a deleterious or miasmatic atmosphere. In what is this vigour or courage supposed to consist? I believe it can only be answered in the negative way. It is not placed in the animal functions, nor even in the state of the sensorium, nor does it depend upon the weight of muscles a man is possessed of: neither has it any particular relation to sex, or age.

72. But the deleterious effects of the air may be resisted, by guarding against the ordinary concurrent causes of excitability or predisposition; such as cold, intemperance, excess of other indulgences, &c. all tending unfavourably to dispose the body to its operation: and therefore much may be done on these points, presuming every degree of fear to be absent,

by attention to regimen and diet: Yet I should apprehend, spight of the strictest attention to counteract all the predisposing causes to fever, that miasma would be constantly generated, if it existed at all, or if it was so readily multiplied, as teachers have advanced, by a fermentative process in the human body. I shall find occasion to speak more at large on the subject of fear, as a remote cause of fever, when I notice its immediate connection with, and operation on, the sensorium and nervous power.

SECT. III.

ON THE AIR, THE PULSE, AND ANIMAL HEAT, AS CONNECTED WITH FEVERS.

73. The influence of the AIR in the production of fever, merits a few select observations. The variety of epidemics is not so great as many writers have supposed, so as to render the problem, which teachers have advanced, altogether inexplicable. When an epidemic fever affects a great number of people at the same time, it must be occasioned by one common cause, namely, either by the condition of diet, or the state of the air. But the variety of aqueous drinks, our common beverage, is not so great as to be a cause of endemic, much less of the apparently epidemic, fevers. It is however more likely, that in the time of famine, many things may dispose to the propagation of either atmospheric miasmata, or human effluvia; as such a lamentable season would, in a general way, give rise to every evil, that would naturally follow at any other period, from poverty and uncleanness. But though these are circumstances of the most unwholesome tendency, we must, after every investigation and enquiry, seek for the general cause of malignant-epidemic-fever in the state of the atmosphere: particularly in a suddenly raging pestilential season. The enquiry is however still *sub judice*, and

therefore with propriety I may notice the prevailing contentions of the present day on the continent of America. Dr. STRINGHAM, of New York, does not agree with the Editors of the Medical Repository, “that the yellow fever of that climate was produced by a modification of the constituent parts of the atmospheric air; and that the change was first evinced by its influence on other diseases.” This respectable professor, from his experience, and his experiments, could not discover any change in the atmosphere of New York at the time of the fever, that is, so far as respected the atmospherical proportion of oxygen, azote, and carbonic acid; there being, to his observation, no perceptible difference between the atmosphere of such infected situations, and that of places which were several miles distant.

74. Other most ingenious and experienced pathologists have differed on this subject. Some have supposed fever to depend more upon the insensible than the sensible qualities of the air: but from our knowledge of the latter we are best able to draw our conclusions. Fever is therefore more generally supposed to arise from its sensible qualities, as its heat, coldness, dryness, and moisture: but it may also, with the aid of concurrent remote causes, be excited by its insensible qualities, namely, matters with which the air may be impregnated: yet such may be no ways concerned with endemic malignancy. The properties of the air termed gravity and elasticity, have also been considered as having no share in the production and support of genuine fever: And of the sensible qualities, heat, in conjunction with moisture, has been considered as the chief cause of pestilential fever. I shall, in a future part of my work, have occasion to notice the particular new opinion of Dr. DOMIER, respecting the cause of the pestilential state of the atmosphere.

75. It has been advanced, that a dry season, by diminishing exhalations, prevents the evaporation of noxious effluvia from the earth; and may therefore occasion the most healthy season. This may in some climates be the case: but I shall,

in its proper place, have occasion to observe, that great stress is to be laid on the influence of heat alone in tropical climates, at the commencement, as well as during the raging, of an epidemic. After all writers have given full conviction, that pestilential fevers, which have continued to return violently, for some successive years, at particular seasons, were always modified or varied by the vicissitudes of the season, and the temperature of the air. It is therefore pertinent to my subject to introduce the following facts, from which Dr. RUSH has inferred, “that the yellow fever is propagated by means of an impure atmosphere, at all times, and in all places.”

1. It appears only in those climates and seasons of the year, in which heat, acting upon moist animal and vegetable matters, fills the air with their putrid exhalations.

2. It is unknown in places, where a connection is not perceptible between it and marshes, mill-ponds, docks, gutters, sinks, and other sources of noxious air.

3. It is destroyed, like its fraternal diseases, the common bilious and intermitting fevers, by means of long continued and heavy rains.

4. It is completely destroyed by frost.

“These principles, according to Dr. RUSH, explain several facts, in the history of the disease, that are inexplicable upon the principle of its specific contagion.” (Ed. Med. Journal.)

76. On the whole it appears, by the writings of scientific pathologists, that the variety of epidemics arise from one or more of the following principal remote causes, namely, cold, marsh effluvia, or human effluvia; and that they become more or less virulent, according to the concurrence of ordinary and occasional causes. I shall beg leave to add my opinion, that heat alone will be found equally entitled to the distinction of a principal remote cause. We are, however, to consider contagion, generally speaking, as admitting, with propriety, of being divided into specific and common: But this division will not account for every variety; and it is from the almost indescribable admixture of other operations on the human body, that the innumerable species of fevers arise.

77. There are few, comparatively speaking, which occur from specific contagion; but they are well known, and scientifically treated; such as the small-pox, measles, chin-cough, &c. and have been considered “as produced by *secreted* matters.” We remain yet out at sea respecting the specific nature of the *truly pestilential* fevers, of which kind we are not to reckon the gaol, ship, or hospital fevers, they being very limited in their communication from one person to another, according to Dr. RUSH, “as arising only from *excreted* matter,” as human effluvia (70). It will afford me much satisfaction, if, by my intended view of *these disorders*, I should throw any better light upon such hitherto destructive maladies; so often fatal to the human race in various quarters of the world. I feel, however, clear of one thing, that it is only from an accurate observation of the various appearances which the different fevers, in different climes, assume, joined to a proper enquiry into the mode of operation of all the remote causes, that we can ever be directed to the best and most certain method of cure, on any occasion in future, which may be similar to what so lamentably occurred in the autumn of the year 1804, at Gibraltar.

78. The PULSE, from the changes it is subject to, forms one of the principal characteristics of fever, and the circumstances pertaining to it most worthy of consideration are, its velocity, frequency, strength, fulness or size, tension, and regularity. A quick, and a frequent pulse, are not easily distinguished from one another. A frequent pulse is not alone a mark of irritation, for a state of debility will generally produce it. A pulse of 120 in a minute, does not portend danger. So also HALLER thought: but if it exceeds 120, it is a mark of irritation, with considerable debility. It has, however, been occasionally found in fever not to exceed much its ordinary frequency in health. But surely the case which Dr. MONRO mentioned to CULLEN, of a man’s dying of a fever, whose pulse had not exceeded 80 in a minute, must have been

a wrong diagnosis. I should have suspected some direct cause of death on the brain, in such a case.

79. The frequency of the pulse in fevers, according to the writings of some pathologists, is not to be depended upon. From the immense variety of fevers, it must be expected greatly to differ in some constitutions. Most practitioners would suppose, that SAUVAGES had certainly erred, when he said, “that the pulse in typhoid fevers is, in respect to strength and frequency, much the same as in health.” I have always found the peculiar frequency and nature of it, in the low nervous fever, particularly characteristic. CULLEN very justly observed, that there is a species of fever called the typhus comatosus, wherein it is slower than in health. From some cursory observations, which I have made in practice, I shall have occasion hereafter of noticing this particular exception of CULLEN.

80. The ANIMAL HEAT of the human body is so much connected with febrile states of the system, that a few observations may very properly be introduced. For a long time our theories, concerning animal heat, have been thought very inconclusive. It is however every where evident, that it is connected with the motion of the blood, and in fevers is certainly dependent on it. Increased heat points at the excess of stimulant power. Coldness of the body is a strong mark of debility, in some function of the system; and when accompanied with horror, is a symptom of febrile irritability. Sweating is an ambiguous sign of febrile heat. It has been thought to have its origin from nervous excitement, as well as from irritability of the heart and circulation; and when perspiration is very profuse, with even a coldness on the surface of the body, I believe it has its foundation in an opinion I have long entertained of a supposed cutaneous generation of animal heat, which opinion I published in my Treatise on Cutaneous Diseases fourteen years ago, and have seen no reason since entirely to alter it, nor did I ever meet with a fair confutation of it, in any periodical, or other work. In fact,

Dr. CURRIE has advanced, “that in the *production* of sweat itself” previous to its appearance on the surface, “a degree of heat is absorbed.” The variations of heat and sweating in fevers, much merit the attention of practitioners; and have, most assuredly, some connection with some disturbance of the healthy function of the skin. It is, therefore, extremely probable, since the attention of the chemical philosopher has been of late years so much given to the investigation of the cutaneous actions and excretions, that much useful information will at last be collected on the subject. I have read with much satisfaction, Dr. GEORGE KELLIE’S “*Historical and Critical Analysis of the Functions of the Skin*,” as an original communication in the second number of *The New Edinburgh Medical Journal*, in which the ingenious writer has briefly brought into one view, the different opinions and experiments of the chemical philosophers of the two last centuries, as well as of the later experimentists, LAVOISIER, INGENHOUS, PRIESTLY, JURINE, CRUICKSHANKS, SEGUIN, ABERNETHY, and, “as the *last* experimental philosopher, who has paid particular attention to the subject,” SPALLANZANI. There is reason, therefore, to expect improvement in this part of medical philosophy.

81. I shall conclude my present observations, as connected with the general pathology of fever, with a few remarks on the doctrine of critical days; and particularly, as I believe there are many practitioners still watchful of them. They do not so frequently occur, or are less obvious in this country, than they were among the ancients in the more southern climates. This is to be attributed to that miasma, which has been thought to produce in those climes the endemic fevers so copiously, being not so much generated here. In this country they are obscured by the prevalence of the inflammatory diathesis, as well as by the debility attendant upon the fevers of a typhoid type. It therefore should be noticed, that from the irregular varieties of fever in this country, the several *crises* are not always solutions of the disease; but they may be

said to be chiefly indicated by a gentle sweat, the going off of the delirium, moderate sleep, the crust falling from the tongue, and a moderate return of appetite. The urine also points out a change at the critical period of the disorder.

SECT. IV.

ON THE PRESENT GENERAL METHOD OF CURE IN FEVERS.

82. I shall next bring together some remarks on the leading principles of cure, and their appropriate remedies, which my reader will find of use towards the more readily entering with me into the future investigation of the Gibraltar disease. I feel it behoves me to say what has been the general practice in contagious and other fevers, that I may make the proper application of the science to the treatment I shall hereafter recommend.

83. As in the administration of remedies to relieve and cure, we have in view the particular species of the disease, they all may in some measure be looked upon as specifics. The practice of medicine rests upon one of two principles, both of which apply to the situation as well as danger of some cases: namely—we either employ such medicines as excite, favour, and support the operations of nature, (the *ΑΥΤΟΝΡΕΙΤΕΙΟΝ* of the schools), or we use those calculated to obviate or remove the causes of disease, which have a tendency to induce death. On the first principle we should only rely, when we know not what else to do; and on it we may sometimes rely without doing much besides, except by regimen. But there can hardly be any one well grounded in the science, who, if he could distinguish the first, would not be equally ready to apply the second to practice. CULLEN proceeded always upon the second plan; and therefore had in view, consistent with his pathological opinions, either the taking off the excess of sti-

mulant power, or the obviating the tendency and operation of the sedative and septic powers, or principles. I shall commence my remarks on the leading remedies of fever, with brief observations on blood-letting.

84. In general, people in cold climates and seasons bear venæsection better than in hot ones: and yet I have reasons for here observing, that it may be fairly admitted in the fevers of tropical climates at the first attack, even supposing them to arise from miasma or contagion, if there should be the least ground for suspicion of the topical action of the pestilential atmosphere: But unless that be the case, bleeding is not generally admissible in fevers arising from contagion or miasma, such as the gaol or pauper fever. Further, we may hesitate less on such occasions between the age of puberty and thirty-five, than earlier or later in life, though there must be cases, according to sex and other circumstances, which may be found exceptions to this general rule (46).

85. Fevers, combined with either an original or acquired inflammatory diathesis, have been sometimes protracted to a great length, for want of one bleeding at the commencement, and will sometimes not give way until it has been once performed, or that a spontaneous hæmorrhage has taken place. CULLEN gave a caution in regard to venæsection in epidemics. He said “that it was necessary for us to observe their nature and tendency, before we made our prognosis respecting its admission, because some symptoms will indicate, whilst others would forbid, this evacuation.” This remark applies strongly to what I have said above: and he has further strengthened my opinion, from the observations I have made in practice, by himself remarking, “It is not the frequency, but the fulness and strength of the pulse, which indicate bleeding; as also does the heat of the body, when considerably increased.” Here I must interpose my opinion, by in turn observing, how necessary it is to discover, that the heat does not arise from nervous causes, or too much bed-covering. Dr. CULLEN also added, “that whenever there was a topical determination in

any part of the system, bleeding was necessary, whatever might be the epidemic.”

86. *Delirium animi* has sometimes deterred from a second bleeding. Hence therefore it may be observed, that if a bleeding has been performed upon a constricted hard pulse, and after the operation it becomes fuller and stronger, a repetition is indicated; but if it remains still weak and small, after the patient has entirely recovered from the delirium, then we are to proceed with the utmost caution, and can hardly venture on a repetition, though other symptoms seem to urge it. Even the inflammatory crust on the first blood does not always justify a repetition of it.

87. A small quantity of blood from a large vein, by a large orifice, has much better effects than a larger quantity from a smaller orifice; and as blisters are stimulant in their effects, venæsection should not be determined on or performed, while their action continues in full force, on account of their effect on the circulation. The use of refrigerant medicines, as the acids and neutral salts, may very properly supply the place of venæsection, when we hesitate to advise it. Then nitre in proper doses is one of the most powerful febrifuges, and in irritable habits may be joined with opium.

88. The promotion of sweating at the beginning of fevers has been recommended; and many cases on record of a dangerous tendency seem to justify the practice. Is however the sweating regimen in fevers always useful and safe? It is a question of the utmost importance, and merits some cautionary remarks. Sweating certainly does not always cure them; though MORGAN, PRINGLE, and others, have thought, that sweating at the attack of most fevers, and even of the gaol fever, has cured: and PRINGLE asserts so, even in the second stage of the latter fever. CULLEN admitted, that certain diseases may be cured by sweating alone, as the plague and sweating fever: but that it would sometimes prolong and aggravate the disease. However, sweating at the commencement

of inflammatory fevers, when not final, is always hurtful; and in such cases is best premised by venæsection, and should at all times be produced with gentle stimuli, and as little heat as possible (46.) The frequency of the pulse should not be much increased, and head-ach should not be caused, by the means used to bring about the sweating. Writers on the plague have observed, that a gentle diaphoresis is the best; but it should be properly continued; and to be efficacious should be universal. Sweating produced by a heating regimen will certainly increase or aggravate all topical affections. It must also be brought about with very great caution, when the septic power may be prevalent; which is readily the case in the plague, from the operation of atmospheric heat.

89. Blisters are the next interesting article, meriting consideration in the treatment of fevers: and their general operation renders them an object of importance in the cure of all pyrexial diseases, as well as in some others; though the theory of their operation has been considered vague and uncertain. They relieve local congestion in inflammatory affections: they are useful in nervous complaints. HUXHAM recommended them in nervous, as well as the advanced stage of putrid, fevers; and DE HAEN advised them, whenever the *vis vitæ* was weak. PRINGLE thought they had the best effects after venæsection: LIND has advised them in the gaol and contagious fevers; MONRO also still more warmly recommends them on the same occasions: but with more caution in the administration of them. In regard to the best situation for application, the nearer they are to the part topically affected the better: but in general fever they are to be applied where most convenient. CULLEN would have always preferred the head, though they are slower of action there. Next to the head, the arms and thighs were thought by him best, because on those parts their action is quicker, than on the legs.

90. I have been long in the habit of constantly recommending the Spanish fly-blisters to be sprinkled with powdered camphire; unless I wished, as for instance in paralytic affections,

to have the full stimulating effects of the cantharides. For general purposes, I strongly advise the application of the camphire to the surface of the plaister; as from it I think three advantages flow: 1st, that their action in producing the serous effusion is évidently hastened: 2dly, that the painful state or degree of inflammation is in a great measure prevented: and 3dly, that the subsequent strangury is often less distressing, if not wholly guarded against. From these latter advantages, blisters may be suffered to remain longer on than formerly, which allows them to relieve more certainly any local congestion, particularly if the cuticle remains attached, as a defence to the inflamed rete mucosum. Next to blisters sinapisms may be recommended, but their vesications, or excoriations, heal much slower than those from cantharides.

91. Warm bathing is the next remedy demanding consideration, as importantly connected with our subject. This has been much used in the treatment of fevers in Turkey. In this country recourse is had more frequently to the pediluvium and semicupium. GILCHRIST has recommended the general warm bathing, from proofs of its utility sufficiently satisfactory: but he obviates the inconveniencés, which CULLEN pointed out, as arising from the insensibility, and helpless state of the patients at the time, by observing, that it is a cordial, and that they therefore bear the erect posture better than could be expected, in the removal from the bath. But surely it might be so managed, that the sick person could be placed in the bath, without being raised erect. CULLEN was disposed to substitute the vapour bath as most convenient, and even thought fomentations properly managed, and continued an hour at least, had the like good effects. However, in whatever way we have recourse to warm bathing, its good effects may not immediately follow its operation; neither is it always attended with a final solution of the disease; but it mitigates the symptoms, and may put matters into such a train, as to bring it to a more speedy termination.

92. I think the pediluvium, or fomentation of the lower

extremities, might prove so importantly serviceable in epidemic and pestilential fevers, in which the sensorium seems so universally to suffer, that I shall mention what Dr. CULLEN said on this subject. “Either of them is of especial service, when there is a determination to, and irritation of, the brain; and of course should be employed, when there is either stupor or delirium; accompanied by subvultus tendinum, and a small, weak, and frequent pulse: And, when there is little or no inflammatory diathesis, the heat thus applied will do no harm, but will be attended with great benefit by removing spasm.” He further added, “But when there is a phlogistic diathesis still remaining, and either creates uneasiness upon the application, they may then do harm by increasing the heat of the body, and forcing out a sweat at an improper time. This most commonly happens, when we have recourse to either of them too early in the disease.”

93. On another occasion, speaking of warm bathing in general, he said, consistent with his own pathology, “that it took off the atony and spasm from the surface of the body, and removed the irritation of the brain; that it induced sleep, and a gentle moisture on the skin, and thus produced some remission of the fever.” And upon the whole he observed, “that the pediluvium,” to which he was partial, “was extremely useful by moderately relaxing the system: and especially by removing the irritation on the sensorium, whether arising from the sanguiferous or nervous system.” The objection which Dr. CULLEN made to the application of it, when there was any degree of the diathesis phlogistica, might I think be removed, by cautiously attending to the proper temperature of the water, that the patient might experience soothing, not stimulating, effects from it: But it is most probable, that when it has been striking, venæsection will have been premised.

94. I shall next offer a few remarks on the excess of the sedative power, or principle, in the system, connected with fever. This most particularly relates to the state of the sensorium, and the origin of all motion; it must therefore be readily

conceived, that it will be difficult to remove or obviate it. When the debility of the nervous system forms the idiopathic affection, it may be capable of being removed by an encreased action of the heart and arteries. The certain balance however, and connection kept up in a state of health (22) between the nervous and sanguiferous systems, renders it uncertain, and very difficult to explain, how and at what time they become mutually affected, under morbid appearances of the whole habit.

95. If we consider an increase of tone in the sanguiferous system, as an excess of stimulant power, we may justly consider the loss of that tone, as arising from debility, an excess of the sedative power. CULLEN observed “that nobody doubts that in the plague, where the debility is most remarkable, there is considerable loss of tone in the arterial system.” “Upon dissection,” he said, “the heart is found greatly distended beyond its usual tone, and its debility, which is evident, may be supposed to have been productive of that general loss of tone, though the latter might also arise from a putrid solution of the blood.” This mode of reasoning applies very properly to that general state of the low nervous pestilential fever, which is idiopathic, and caused by the deleterious and sedative action of a miasma: But I shall have occasion hereafter to explain the apparent excess of a sedative power over the nervous system, in epidemic fevers, on another principle.

96. The removal of the sedative state of the system is to be brought about by the use of appropriate remedies: and as the excess of it is connected with, and hinges on, the general debility of the system, the means proper for the removal of the one counteracts the fatal tendency of the other, and they may be divided into four heads, namely, the application of cold; the use of tonics; the use of stimulants; and the use of antispasmodics. The first may be applied either externally or internally: But as cold drink is a more powerful remedy than warm ones, and capable of producing greater effects, it may,

if imprudently used, do a great deal of mischief; and therefore ought to be had recourse to with much caution. It is to be avoided, where there is the least inflammatory diathesis, or any suspicion of topical affection. As to the external use of cold water, it has ever been considered as a remedy in fevers of the putrid and petechial kind; and CULLEN had no doubt of its being a very useful and universally applicable remedy, both in the nervous and putrid fevers. DE HAEN found the washing the body all over with cold water of great service. It was recommended by CULLEN, that cold water, both externally and internally, should be used at the height, or towards the end of the febrile paroxysm or exacerbation, in order to induce sweat, and thereby a final solution of the disease.

97. I cannot pass unnoticed in this place, the sentiments of an author on aqueous practice, whose work has been stamped “as the most valuable which has ever been published;” I mean, “The Medical Reports on the Effects of Cold and Warm Water, as a remedy in Fevers and Febrile Diseases,” by Dr. JAMES CURRIE, of Liverpool. This intelligent writer has very largely indeed considered the subject. I shall, however, only here notice a few of his observations, to shew to my reader the extent and importance of his reflections and experiments. Dr. CURRIE has ventured to affirm, “that, used in the first three days of fever, the cold affusion very generally stops the disease; that the same happy effects sometimes follow its use on the fourth, or even fifth day, but seldom later; that even in the subsequent stages, where the heat continues præternaturally great, and the skin dry, it is of great and manifest advantage, almost immediately relieving the most distressing symptoms, particularly restlessness and delirium, and conducting the disease *to a safe and speedier issue*.” In the critical analysis of the above work, in the first number of the New Edinburgh Medical Journal, is the following passage. “The medium temperature of the water in Barbadoes is 74° or 75° fahrenheit; and this appears equally efficacious as a colder application. In the fevers of the West Indies, it has been found

very grateful and advantageous. However, it did not succeed, according to Mr. M'GREGOR's account, in the fever most common in the East Indies. The failure, in some cases, stated by Dr. MACLEAN, is obviously to be imputed to its improper administration. The period and manner he adopted is quite opposite to the rules laid down by our author. At Philadelphia, the cold bath was used, and recommended in epidemic fever, by Dr. STEVENS; but it was soon abandoned. No thermometrical observations were regarded: its success could only be precarious and accidental, as it was employed in the latter stages of the disease, as a stimulant and tonic. It has lately been used with success in Virginia, according to the rules laid in the first volume of this (Dr. CURRIE's) work."

98. Reflecting on this application of cold water, as an universal practice in fevers, I cannot but think, that at the commencement, it must often with hazard be had recourse to, when the nature of the fever is not known. This appears to have been the fact in four cases of measles, but without injury, according to Dr. CURRIE's report: nevertheless, as much caution seems requisite in the use of cold water, as of venæsection, you may misapply the one as well as the other, and thereby do harm. Where the judgment of the practitioner is so ripe, that a just discrimination can be formed between the idiopathic and symptomatic fever, allowing for the difficulties of the art with every one, I believe, that on some occasions, in all climates, cold effusion or ablution may be of service. I should, however, cautiously trust to its operating with a *magic* charm. Extreme attention must be necessary, when the inflammatory diathesis prevails (96), least, its magic charm failing, a local affection might be either aggravated or occasioned. Indeed Dr. CURRIE does not recommend the use of the cold or the tepid effusion, or cold drink, in fevers accompanied by, or originating in, high local inflammation; but on the attack of fever, for the first, and perhaps second twenty-four hours, symptoms of local affection do not always

appear, and there are some local affections, which are characterized more by low nervous, than by inflammatory, symptoms; such from cold water would be easily aggravated, when we may not be aware. The magic charm here would fail, and it is clear, that the safety of the practice is not fully established, from the Editors of the Journal having observed, “that Dr. CURRIE has not stated the grounds of his objection to the practice in inflammatory cases.”

99. We will pass over the catalogue of tonics and stimulants employed in the cure of fevers, and confine ourselves to a few select observations on wine, as sufficiently answering the intention of pointing out the principles, on which the practice of giving them has been founded, and on this head I cannot serve my reader better, than by giving Dr. CULLEN's few practical remarks on the use of wine in fevers, as a tonic. “Where,” said the professor, “the fever arises from contagion, and there are no symptoms of inflammatory diathesis, or if the patient has been formerly habituated to it, we may indulge him with it the more early; and when the patient finds it the more and more grateful and refreshing, and delirium is diminished by it, then we may certainly administer it liberally; but when the patient has a distaste for it, is not refreshed by it, and finds himself heated and made uneasy from it, and even loathes the taking of it, then I would advise the omission of it altogether.” In thus submitting to the patients feelings, Dr. CULLEN always found an advantage, and therefore recommended to his hearers these precautions. Furthermore, Dr. CULLEN observed, “that the weaker the wine, and the less impregnated with brandy, the better;” and that he had “always found the claret and rhenish much better than the Spanish and Portuguese wines, and that at the first using of any of them, “it was better to dilute them.” Sack-whey, he thought, was never a proper cordial; and negus, though better, fell short of plain wine in very small quantities, or even mixed with cold water; the last he considered as the most refreshing,

and observed, “that in all cases where wine was proper, cold water might be used with safety.”

100. Many respectable opinions might be referred to in confirmation of these sentiments, from wine having been generally admitted as the most safe and effectual stimulant in fevers. I shall conclude my remarks on wine, with what PRINGLE has observed, “that there is nothing comparable to it in slow fevers, unless it should excite delirium, and then it may be deemed hurtful.” “It is best therefore not to have recourse to it too soon in the complaint, perhaps should not be thought of sooner than the second week ;” nevertheless, I have myself found, on some such occasions, brandy diluted with water, a better stimulant than wine.

101. Antispasmodics formed the fourth division of our present observations. It is difficult to determine their proper place, and to ascertain the propriety of their use in fever. The different articles of this class of medicines are numerous, but opium is the chief of them, and I shall merely confine my remarks to its operation in febrile disorders, as applicable to our future enquiry. Most practitioners have admitted, that there is in opium a combination of sedative and stimulant powers: this occasions the necessity of much caution in its use.

102. Opium is improper in the beginning of all fevers, and more particularly hurtful in inflammatory fevers; there is, nevertheless, no bad quality in the medicine, but it has been the mal-administration of it, which has shaken its utility, and sometimes brought it into disrepute. It may, however, be sometimes useful as a stimulant, as well as the bark, in some kinds of inflammatory affections; but it is particularly so in cases of irritation without inflammation; here it is proper to remark, that in small doses it is stimulant, and in large doses sedative.

103. It is said, that in mania, without increased impetus of the circulation, it has been found highly beneficial. CULLEN had generally experienced its good effects in that kind of

delirium, which occurs in the advanced stage of fevers, and then in larger doses, as the best stimulant with a sedative power. In the earlier periods of the disorder, when the fever has been of a mixt character, the synochus for instance, I have thought it very serviceable joined with small doses of nitre. It was also an observation made by CULLEN, “that opium joined to neutral salts, would produce a sweat of the most favourable kind.”

104. The dose of the opium must be proportioned to circumstances, but should not, and need not, be large in any case of fever. It may, however, be fairly said, from the variety of opinions entertained respecting the use of opium in fevers, and inflammatory affections, such as rheumatism, small-pox, &c. that there is no absolute rule in physic: but I believe most practitioners would agree with me in thinking, that it would be prudent to premise bleeding in inflammatory affections, if the case should otherwise seem to demand it, before the use of opium is admitted.

105. I shall briefly observe, as connected with my enquiry, and from being the next best antispasmodic in common use, that camphor is employed with advantage, as an undoubted sedative in fevers, from its primary action on the nervous power, but is not a stimulant, as opium. It has been thought by some a refrigerant, but it is uncertain in its effects; it is sometimes well adapted to the maniacal states of fever, and is also of service in the subsultus tendinum, and irritation of the brain without fever.

SECT. V.

ON THE EXCESS OF THE SEPTIC STATE IN FEVER.

106. It may be observed, that a proper febrile affection is constituted by the concurrence, or communion, of certain states, or powers, of the system, which have been termed by

the systematics the stimulant, the sedative, and the septic, powers; and it is by the excess of these, conjointly, or separately, that the indirect, or occasional causes of fever, produce a direct tendency to death. I therefore think it particularly necessary to make a few remarks on the excess of the septic state, or power, in the system under fever.

107. The existence of the septic state will be found either in the *primæ viæ*, or in the circulating mass of fluids, or in the excrementitious parts of the body, by the following symptoms. In regard to the first, such a condition of the system is generally accompanied with a sense of fulness, a loathing of animal food, nausea, and vomiting, with an imperfect state of digestion, all which however might arise from debility of the stomach, and therefore these circumstances, in a diagnostic point of view, may sometimes mislead the practitioner; but when such symptoms are accompanied with a disagreeable taste in the mouth, a foetid breath, nidorous eructations, an insatiable thirst, and a craving appetite for all kinds of acids, they are, taken together, unquestionable indications of a putrescent state of the contents of the first passages.

108. The second condition of the system is judged of by the state of the blood when taken out of the veins, or when extravagated, and still apparently connected with the circulation. A loose putrescent character of the blood is marked, by the serum not separating from the crassamentum, or when it does, that it is not of its usual and proper colour, appearing more like a bloody water; or when the serum is of a more yellow colour than common. CULLEN however remarked, “that this last was an ambiguous appearance, for that it would sometimes happen to be so in inflammatory diseases.” And he also pointed out to us, “that as the cohering property of the blood does not depend upon the serum and red globules, but on the coagulable lymph itself, the separation of the lymph, or the destruction of its coagulability, is undoubtedly a mark, according to its degree and combination with other circum-

stances, of either the inflammatory diathesis, or the state of putrefaction.”

109. A judgment has also been formed of the degree of the septic condition, by the appearance of the fluids, as still connected with the circulation, or extravasated. CULLEN on this head has said, but with him I do not wholly agree, “that when blood is poured into the intestinal canal, and voided by vomiting or stool; also when it issues from the nose in drops, which are thin and fluid; and lastly, when it issues from the ears, eyes, and even the pores of the skin, in concurrence with other symptoms of putrescency, they are very strong and certain proofs, that the blood is in a dissolved state, in consequence of the excess of the septic power.” On this principle, the *maculæ lividæ* and *petechiæ* have been explained, but I do not think that they ought to be considered, as always marks of a putrescent state of the system; they certainly will occur sometimes either from a state of congestion, or a weakness of the vascular solids (50 et seq.) Of the yellowness of the skin, in the commonly called yellow fever of the West Indies, as supposed to arise from a septic cause, I may have occasion hereafter to speak.

110. In regard to the third, as to the judgment to be formed of the general state, by the condition of the excrementitious secretions of the body, practitioners in general do not hesitate in deeming the septic principle to prevail, when the *fœces* are black, or very dark-coloured, and extremely fœtid; when the urine is of a dark, reddish, and turbid colour; when the discharge from blistered places bears a strong fœtor; when the perspiration is fœtid, and a cadaverous smell seems to arise from the whole body; but these appearances will certainly and very readily occur in tropical climates, under a plethoric or inflammatory state of the habit.

111. On the whole of this subject, Dr CULLEN observed, when drawing his conclusions from the various symptoms of general putrescency in fever, “that the appearances of it were

always attended with an increased action of the vessels, and evident marks of the action of a stimulus." And from these circumstances he considered, "that all fevers so characterized, especially towards their end, were attended with evident symptoms of the true typhus, such as were, on the whole, striking proofs of the prevalence of the septic power." If this reasoning is admitted, it proves that the change in the fluids may be brought about by the morbid stimulus of an inflammatory diathesis.

112. In the treatment of malignant epidemic fevers, nothing, generally speaking, can be more important, than the obviating or removing the excess of the septic power. A few general remarks on this head will therefore be highly proper. In order to obtain the points in view, our attention must be called to the following indications.

I. The obviating the remote causes.

II. The removal of putrescent matter by evacuations.

III. The use of proper antiseptics, and

IV. The use of tonics.

113. These various objects are effected by shunning all marsh and human effluvia, and particularly the patients own effluvia; by due ventilation, change of linen, and cleanliness of apartment; the use of quick laxative clysters contributes to our purpose, when we dare not venture on internal aperients, for fear of debilitating, and though motion prevents the putrefaction of our fluids, all preternatural degrees of it would increase the tendency to the septic state; but mild diluents, and gentle laxatives may, on some occasions, be very beneficial, and sometimes preferable, for the removing foulness of the primæ viæ. The frequent washing of the surface of the body with cold water is in some cases very proper, (96) but profuse sweats should not be excited, because they do not carry off morbid matter, if any such really existed, but tend much to debilitate the system. The end we have in view is further, and even better, answered, by the proper application of cold or cool air, as a tonic and antiputrescent remedy.

114. Further, to correct the septic tendency, vegetables are well suited : but for this purpose their place is better supplied by the fossil acids, as they not only alleviate, but correct, that morbid state of the fluids, and which is chiefly prevalent in the *primæ viæ*. At the same time that we, by these means, attend to the expulsion and correction of excrementitious matters, as the nidus of putrefaction, we must prevent the further generation, as well as continuance, of them, by the admission of wholesome and nutritious aliment. But when there exists a very high degree of the septic condition of the system, accompanied with great debility, we have then to rely principally on the application of cold, conjointly with the judicious use of wine and Peruvian bark. For, as CULLEN observed of the bark, “its action as a febrifuge depends upon something more, than its antiseptic quality.” I do not know of any article of the *materia medica*, in its nature truly harmless, that requires more caution in the administration of, than the Peruvian bark demands in fevers, as well as in some other affections, both in regard to the quantity to be used, as well as to the occasions for using it ; and the more so, because it is a remedy in such general use.

SECT. VI.

ON INFLAMMATION, AND ITS TERMINATIONS.

115. Inflammation being so much connected with most of the epidemic fevers, as appears from the histories in general we have had of them, I shall next proceed to some observations on the subject of Dr. CULLEN’s *phlegmasiæ*, and of the inflammatory diathesis : And as this diathesis so much prevails in general in febrile disorders, some brief remarks on the difference between the *febres* and *phlegmasiæ* of CULLEN’s nosology, are necessary to the object of my ultimate enquiry, and to the readily comprehending my intended illustration.

116. The PHLEGMASIÆ, or fevers with local inflammatory affections, agree in many respects with true idiopathic fever. Their *grand* difference lies in this circumstance, namely, that the former are always attended with the inflammatory diathesis, and some *decided* topical affection; whereas, the latter is considered only as arising from a general inflammatory temperament. But I believe an apparently inflammatory fever has often occurred, and been cured as such, without the topical affection having been suspected.

117. The predisposing or exciting causes of the phlegmasiæ are, the plethoric habit, and tense, firm, and rigid fibres. From among the occasional causes may be mentioned, as the principal ones, cold; direct stimuli, which act upon the vis nervosa; and the injudicious use of tonics, too frequently the consequence of domestic practice. The immediate symptoms are, an increased impetus and action of the vessels; an increased tone of the system, and strength of fibre; but more especially a hardness of the pulse. The cure is principally effected by the immediate power of venæsection, as capable of inducing local, as well as general, relaxation.

118. On this general outline Dr. CULLEN founded his opinion, saying, “that both from the causes, symptoms, and cure of inflammation, we are authorized to conclude, that it is dependent on the state of the vis insita, or inherent tone of the system.” On this ground therefore it would appear, that the pyrexia of inflammation, or of local inflammatory affections, depends on the vis insita, and the pyrexia of true idiopathic fever, on the vis nervosa. It may further be observed, that inflammation terminates either by resolution, or suppuration; and that the true phlegmasiæ are never contagious, but when they are symptomatic. This leads me to consider a little the ordinary specific terminations of inflammation.

119. The various species of inflammations are generally divided into cutaneous, visceral, and articular. They were all supposed by CULLEN to have been at their commencement

membranous, and afterwards to become parenchymatous. When inflammation does not terminate by resolution, but leads on by its continuance to suppuration, *pus* becomes the product of it. In regard to its formation, writers have differed much: but I believe it to be now pretty generally admitted, that the serum of the blood of a healthy person, kept in a proper warmth for some time, will be converted into *pus*: or what ordinarily appears in the form of *purulent* matter. Thus heat and stagnation seem unquestionably to have co-operated in its production.

120. This occurrence, or product, has often given rise to difference of opinion, and much embarrassment, in forming a certain and just diagnosis of pulmonary complaints. For *pus* has been known to have been spit up from the thorax, for a long time together, when on dissection no marks of erosion or rupture were found in the lungs. The recovery of such cases has therefore given occasion for thinking, that the true pthisis pulmonalis has been cured, when in fact no ulceration on the lungs had taken place (11). In like manner, an effusion of serum on the brain may, under peculiar circumstances of constitution, lead to the formation of *pus*. It may also be remarked, that when laudable inflammation is absent in wounds and ulcers, suppuration does not take place.

121. Another termination of inflammation, is by gangrene, or sphacelus. Gangrene may arise from atony of vessels, without any preceding effusion into the surrounding cellular membrane. There is no difference between gangrene and sphacelus, but in degree. Inflammation may also terminate by such an effusion of red globules, as to destroy the tone or texture of the part, before suppuration or putrefaction can take place; and when such happens to a vital organ, it becomes speedily fatal. In this way we may account for the sudden suffocation in the case of peripneumonic inflammation: and I believe such a termination may have often happened, when not suspected, in maniacal, phrenitic, and *some other* local affections.

122. PHLEGMON and ERYSIPELAS are sometimes connected with internal inflammatory affections, as will hereafter be noticed; but they generally form different species of cutaneous inflammation. Yet they may in some cases be joined. The erysipelas, which forms the eruption in consequence of fever, is very properly considered as an erysipelas phlegmonodes; for the *acrid matter* is commonly communicated to the deeper seated vessels, and occasions suppuration, in conjunction with erysipelas. The furunculus therefore, on the other hand, may be called phlegmon-erysipelatodes, because a particular matter is lodged in the cellular membrane, in conjunction with the suppuration. These remarks on this particular kind of cutaneous inflammation become necessary, from its being so often connected with the internal inflammation, of which I shall have occasion hereafter to speak.

123. I shall next set down, as referable to my future enquiry, a few practical remarks on the general treatment of the inflammatory diathesis, when locally affecting the various parts and organs of the human body. In cases of decided topical inflammations, topical bleedings are preferable to general bleeding: and of these, cupping and scarifications have been thought more convenient and less fatiguing than leeches. In that most serious of all internal inflammations, the inflamed condition of the meninges of the brain, aperients are of much use, though they should not supercede the employment of venæsection, which may be pushed as far as to produce revulsion.

124. EMETICS have been recommended in some inflammatory affections; but they are not necessary, and may do harm in some symptomatic fevers: And here it is applicable to our future investigation to observe, that Dr. CULLEN could not have had any proper or fixt idea of the plague, different from the common acceptation of it, from the advice he gave respecting the use of emetics in the disease, for he said, “that so great was the analogy between the plague and other fevers, that emetics were admissible even unto full vomiting at the

first, and afterwards in nauseating doses.” But as the Doctor objected to the use of emetics in cases, where there was any reason to suppose, that any considerable effusion of blood to the internal parts existed, I shall hereafter endeavour to point out reasons, why they are especially inadmissible at the commencement of the plague, and epidemic fevers in general, from the symptoms so generally observed at the attack (35, 38). Neither does the new opinion of Dr. RUSH, that the plague and yellow fever are of the same nature and origin, alter in the least the tendency of my enquiry.

SECT. VII.

ON THE VIRULENCE OF EPIDEMIC FEVERS.

125. I think it pertinent to my subject, to add a few observations on the virulence of epidemics, as connected with human effluvia. It was observed by Dr. CULLEN, “that though effluvia from the human body in a sound state are very innocent and mild, it was generally admitted, that, when accumulated and retained, they became extremely unwholesome, and at last acquired the properties of a most virulent poison.”—And that it was also known, “that by communication with other bodies, their virulence became increased.” He therefore remarked, “that most epidemics are propagated by a fomes, which nourishes itself, so as to become highly virulent, and that such diseases, at the first breaking out of a pestilence, attack with the greatest violence, becoming milder as they grow more general and epidemic, and are at the last propagated only from the human body.” This is a matter meriting the most serious consideration.

126. On these grounds, however, Dr. CULLEN concluded, “that the effluvia, which arises from contagion, are milder in their effects, than those generated by a fomes.” And as a proof of the greater violence of diseases when introduced by a fomes,

he took notice of the infectious fever at the Old Bailey some years ago, where the fomes was introduced by the clothes of the prisoners at the bar. He observed, "that out of forty persons who were so infected, not three recovered." Whereas he had not learnt, "that any person afterwards caught it of those, who had been infected in court, though there had been every reason to believe, that they all had been properly attended by physicians, nurses, &c. on that occasion."

127. On this principle he accounted for the mildness of the small-pox from inoculation, conceiving it to be owing to the infection being communicated from the human body, and not introduced by an atmospheric fomes, as in the natural way. There is something surely too specific in the character of the small-pox, and even of the proper contagious diseases in general, to admit of this explanation to its full extent, I believe however, that it will hold good in the case of the *endemic* pauper fever, so frequently met with in this metropolis, and other large cities. Here I may again take notice of the opinions of the learned professor of medicine, in the University of Pennsylvania, Dr. BENJAMIN RUSH. They will be found interestingly connected with our subject, and are related in the following passage, selected from the Critical Analysis in the third number of the New Edinburgh Medical Journal. The professor's paper on the yellow fever, as given originally in the sixth volume of the American Medical Repository, has been thus noticed. Dr. RUSH observes, say the Editors, "that when fevers are communicated from one person to another, it is either by means of secreted, or of excreted matters. In the former way the small-pox and the measles are communicated, in the latter, the gaol, ship, or hospital fever" (77). The perspiration, by acquiring a morbid and irritating quality, in consequence of its stagnation and confinement to the body, in a tedious gaol fever, is the principal means of its propagation; it is confined to sailors and passengers in foul and crowded ships, to prisoners and patients in foul and crowded gaols and hospitals, and to poor people, who live in small and

damp confined houses ; it prevails chiefly in cool and cold weather, is never epidemic, and is not communicated more than a few feet from the sick, unless by means of clothes worn by them. It has been but little known in the United States since the revolutionary war, but has now and then appeared in ships crowded with passengers, from different parts of Europe ; it is a common disease in the manufacturing towns in Great Britain, and has frequently been confounded with the West Indian and American yellow fever, which differs from it in prevailing chiefly in warm climates and seasons ; in being the offspring of dead and putrid vegetable and animal matters ; (74) in affecting chiefly young and robust habits ; in being generally accompanied with a diseased state of the stomach, and an obstruction or preternatural secretion and excretion of bile ; (35, 39) in terminating most commonly within seven days ; in becoming epidemic *only* by means of an impure atmosphere ; and in not furnishing ordinarily those excretions, which when received into other bodies, re-produce the same disease." This diagnostic account of fevers thus communicated by excrementitious matters from the human body, and contrasted with others supposed to derive their origin from putrid exhalations of the atmosphere, from the sources before mentioned (75), may be well founded, but it does not reach to the extent of the enquiry I have in view, to account for the raging of any epidemic, or pestilential distemper in a hot climate, such as was the late Gibraltar disease, or of any other, which may not be strictly contagious.

SECT. VIII.

ON THE CONNECTION BETWEEN THE BRAIN AND CIRCULATION IN CERTAIN FEBRILE AFFECTIONS.

128. A wish to comprehend in the present undertaking, all those parts of the general science of medicine, which I have

found necessary to, and connected with, the investigation held in view, has induced me to lay before my reader in the following sections, some appropriate remarks on the physiology and pathology of the brain. This design, I flatter myself, will assist the younger members of the profession, in more readily traversing the ground of enquiry with me, as it pertains so immediately to general systematic principles. I shall therefore first observe, that, as defended by the cranium or bony covering of it, the brain is the seat of the sensorium, and the origin of the nervous power, and so intimately interwoven with our very existence in all the functions of life, that there can be no principle in natural philosophy and mechanics, but must refer to some one part or other of the animal economy, and its operations. It appears very evident, that until the middle of the last century, there had been but little progress made in the knowledge of the hydraulic and organic systems, *with which certainly the principle of fever must be connected*, both when the sensorium and nervous energy of the brain, as well as the circulation, are affected.

129. The cranium, agreeable to the opinion of Dr. WALKER, in a late number of the Medical and Physical Journal, is not wholly intended as a defence to the brain from external injuries, but as a boundary, a guard of security, a circumvallation, to prevent any exuberance in the action of the circulation from impairing, impeding, or deranging, either partially or universally, the immediate and established organization of the cerebrum, cerebellum, and medulla oblongata, in any one part of them, they being all liable to be very essentially acted upon by inordinate impulses of blood, from morbid actions or exertions of the heart and circulating system. If we add to this rational physiology, the novel and deeply investigated doctrine of Dr. GALL of Vienna, as made known in the same number of the Journal, viz. that the brain and nervous system emanate as it were from the medulla oblongata and spinal marrow, and that the sensorial functions of the cerebral organ depend, in a particular manner, upon the superficial structure, or organ-

ization, at the cortical circumference of it, as connected with the meninges immediately covering it, and lining in a manner the internal surface of the cranium, marked, as he has pointed out, by depressions internally, and by *elevations and prominences on the outer part of the skull*, thus dividing the whole into thirty-two distinct *organiculi*; can we, after this, wonder at the frequency and universality of nervous febrile affections, and mental derangements, arising either from local injuries partially, or atmospheric changes generally affecting, the sensorium and nervous power, and also the whole of the functions of the system dependent on them? This new opinion, however, embraces ideas almost beyond our comprehension.

130. The immediate relation between the sensitive and nutritive systems of the human machine, is mechanically as well as vitally supported; is not this evident by the increased impetus of the heart and circulation proving the means of restoring the lost or impaired energy of the nervous system? (68, 94). Again, by sedatives proving stimulant to the circulating system, by having first debilitated the nervous system; the effects also of cold on the human body, both as a sedative and a stimulant, prove the same. Thus the brain may be stimulated by cold in a secondary manner, through the medium of the circulation, which so sensibly varies by its operation (66).

131. The general condition of the body, and the performance of its various functions, as well in health as under disease, depends upon the nervous power; the muscular and moving fibres derive their power as certainly from the state and condition of the brain and nervous system, as the circulation does its continuance from the due action of the heart and arteries (23 et seq.).

132. The primordial structure, and component parts of the brain, were more importantly connected with the original organization of the whole system, than the structure of any other organ, from its particular and necessary communion with the heart; and on this, and the distribution of a just and proper *quantum* of blood to the solids, does perfect health

depend (22). While such a perfect communion continues, the human frame may escape every disorder, save what it is heir to from specific contagions (29), during a very long course of life. Professor CULLEN used to say, that he could point out by illustration, “that the health of the animal body depended more on the proper distribution of the blood to its various component parts, than on any other circumstances.”

133. The physiological opinions advanced by Dr. KELLIE of Leith, in his original communication of a case of torpor from cold, in the third number of the Edinburgh Medical Journal, open a new field for pathological enquiry into the obstruse nature of cerebral and febrile diseases, and should therefore here be noticed. His new ideas, respecting the vitality of the brain, have been conveyed to his reader in the following language. He has conceived, that in the case of torpor, the state of collapse, or insensibility, “is in consequence of the connection of the brain with, and its dependence upon, some other of the functions antecedently and more immediately affected; and this function he apprehends to be respiration.” He has supposed, “that a very intimate connection is maintained between the organs of respiration, and the origin of the energies of the nervous system, through the changes produced on the blood during the pulmonary circulation;” “and has considered this dependence of the brain upon the properties of the blood, maintained by respiration, as evinced by a great variety of observations.” He in an especial manner defends his opinion by the experiments of BICHAT, who, he says, “has, in a masterly manner, traced the mutual connection and dependencies of the vital functions, in his admirable “*Recherches Physiologiques, sur la Vie, et sur la Mort.*” This new opinion brings still more home to our reflection the important connection of the brain and circulation, while it places the latter as dependent in some degree upon the passive action of the lungs, from the immediate communion thus supposed to exist between respiration and the sensorium.

134. As the causes of febrile disorders have so frequent an operation on the sensorium and nervous power, I think it proper to mark down some further circumstances, establishing the primordial connection between the physiology of the brain, and circulation. The vessels of an animal body are always full. When the just balance in the necessary distribution of the blood is disturbed, there must be produced in some one part of the body a considerable tension, and this lays the foundation for what has been called a morbid plethora (22 et seq.). By our creation we proceed from a small to a considerable bulk. In the first periods of life, the resistance of the extretories is greater than that of the larger vessels, from the laxity of the solids. As the body increases in strength and age, the solids become more dense, and of course the resistance of the vessels to the pressure of the circulating fluids also increases; and it is then that the plethoric state will appear. This is first distinctly observed about the age of puberty, from the peculiar changes then to take place.

135. The brain must in every sense be considered as the principal organ of the system, for the conducting of which we find, that the extra-determination of circulating fluids is first made there. Thus, the head is the first finished member or organ of the animal body, and from the beginning receives more blood than other parts: and hence its vessels are the fullest. But as it arrives soonest at its *acmé*, so the resistance to the fluids is soonest made there: And this explains why hæmorrhage happens most frequently from the nose, at and about that period of life: after this the balance has a tendency to change, and the plethoric state is liable to be produced elsewhere.

136. When the age of puberty is fully passed, and the growth of the whole system is in a great measure completed, then the plethoric state will be more especially felt in the lungs. These organs will then most readily suffer from congestion, because they regulate the balance of the whole body, and circulate as much blood as goes the round of the whole

system ; at the same time that their vessels are smaller, and weaker. Hence hæmoptoe, or spitting of blood, will more generally take place at this period, than either at an earlier or later time of life.

137. In the first stages of life, the arteries are more lax and distensible than the veins ; so that the effects of the full growth of the body, relative to hæmoptoes, may continue fifteen or twenty years ; and during this period pthysis pulmonalis more generally prevails, from the above cause. But in the after periods of life, the arteries become stronger, and less dilatable, from their constant action ; and thus the balance falls upon the venous system about the age of thirty, or thirty-five years. At and after this period, without external causes occasioning them, hæmorrhages may arise from venous congestion : and they generally first occur, where the circulation is slowest. I believe this may be said first to happen in the system of the *venæ portarum* ; and hence arises at this age the hæmorrhoidal flux.

138. After the middle age of life, the motion of the blood is also very liable to interruption in the nervous system of the brain : for it there wants the assistance of muscular motion to propel it on ; and as these vessels soonest arrive at their perfect acmè, so it is most probable, that they soonest acquire a morbid rigidity. Further, as the circulation through the brain is entirely supported by the action of the heart and arteries, I think it must clearly appear, why venous congestions and hæmorrhages of the *ENCEPHALON* happen so often at, and after, this period of life, giving rise to apoplexies, palsies, &c. and therefore, from the opinion I entertain of the nature of epidemic diseases in general, I believe there may be some just foundation for the observation made by the Editors of the *American Medical Repository*, “ that the diseases of palsy and apoplexy had been the precursors of pestilence, that is, of the yellow fever, at New York ;” and, “ that even such cases had assumed appearances, which more generally belong to *malignant*

fevers:” at the same time it may be fair to agree with Professor STRINGHAM, that such cerebral affections “had not prevailed with an *unusual* and *alarming* frequency,” (Edin. Med. Journal, No. 2) as stated in the Repository. It is much to be wished, that the age of the patients attacked with the above cerebral disorders, had been mentioned, in order to judge, whether it was arterial or venous congestion. When such affections arise from various congestions, the drawing blood by the jugular veins will have much greater effect, than bleeding in the foot, on HOFFMAN’s ancient doctrine of derivation and revulsion: renewing as it were, by the jugular venæsection, the benefit of a juvenile hæmorrhage from the nose (135).

139. The preceding physiological circumstances, respecting the brain and circulation, will be found hereafter much connected with my enquiry, and they formed the ground work of Dr. CULLEN’s cure of hæmorrhagy in general.

140. It will moreover be material to my purpose, to introduce the worthy Professor’s observations on the EPISTAXIS, or hæmorrhage from the nose. “I consider,” said he, “this hæmorrhage as a relief to various disorders, and often a preventative of some, especially those connected with a plethoric state of the brain; for, according to the STAHLIANS, an hæmorrhage from the plexus of arteries, lying so superficially on the membrane lining the cavities of the nose, seems to serve as an evacuation to relieve the brain, as they are not only supplied from the external carotids, but also from the internal vessels ”

141. The evacuation is sometimes arterious, and sometimes venous; and hence the critical terminations of fevers, and some other disorders, by an hæmorrhage from the nose, can be explained; and so nicely is the balance of the circulation kept up in some hæmorrhagic constitutions, that vertigo, head-ach, congestions of the lungs, and even hæmorrhage from the nose itself, have been known to ensue from a sudden stoppage of an hæmorrhoidal flux. The cure therefore of

piles ought very critically to be considered on some occasions (137), on the doctrine of the vis medicatrix.

142. I have heard it observed, that an epidemic fever, attended with delirium, has often terminated by an hæmorrhage from the nose. This circumstance has puzzled those writers, who had supposed, “that fever was connected with the doctrine of coction and crisis, and a morbid matter.” I do not wonder at their difficulty of explanation, and it serves to me as a proof, that local affections of the brain existed formerly, which were relieved by this local profluvium then, as they are at the present day, so as often to lay the foundation of recovery, by the fortuitous removal of a congestion.

143. Diseases of the sensorium may arise either from concussion, or compression; of the brain; the latter from either fractures, or inward tumours, of the cranium, or from over distension of the vessels immediately connected with the medullary substance. An over distension of vessels produces only a temporary, or transitory, kind of apoplexy. Rupture of, or effusion from, them, causes a continued one; and I think it is not conjecturing too far, to suppose, that the brain may be liable to such a morbid derangement, or compression, from the former cause, as to give rise to a febrile apoplexy, or apoplectic fever: of which nature it is probable, that some of the ordinary brain fevers are. While therefore a distension of the larger vessels and sinusses cannot alone compress the origin of the nerves, or nervous influence, a general compression of the medullary substance may arise from distension of the extremities of the smaller arteries and veins, and thus produce more general effects.

144. Effusion of fluids upon the brain, or within the cranium, is generally of more serious consequences, than over distension, and may be either of the sanguineous or serous kind. External violence, causing concussion, or contusion, may occasion an effusion of red globules; or the same may arise, according to cases on record, from a sudden increased impetus of the arterial blood by violent anger, or exercise, or the

impulse caused by maniacal and phrenitic exertions ; any resistance also to the return of the venous blood to the heart, as violent straining, may produce the same. This, by distending the vessels, and causing an accumulation, in consequence of the temporary regurgitation, may occasion a rupture of vessels, or such an over distension, as may never be removed. How far sudden terror may produce similar effects, will be more fully considered in another place (68, et seq.).

146. It appears therefore, that a serous effusion may be the consequence of an over distension of, or resistance to, the circulation in the larger vessels, which effusion, in an ordinary way, is no more than an exhalation from the smallest vessels ; and as lymphatic vessels and glands have hitherto been but imperfectly discovered in the brain, the only way that a due and regular absorption takes place, must be by the inhalant veins, when not morbidly affected by an undue regurgitation, or resistance, through congestion, or inflammation. In short, whatever obstructs the return of blood, from the brain to the heart, must occasion a temporary compression on the brain, and on some occasions leave a congestion, or effusion, which is not afterwards removed.

146. I cannot, however, help observing, that, from the peculiarity in the vascular organization of the brain, and its membranes, it is probable, that the encephalon is much exposed to diseased states, and I believe is more frequently so affected, than is generally imagined. Is there not much reason to apprehend, that the fatality of fevers in general arises too often, from the disturbed state of the circulation through the brain not being early enough suspected, and too much laid to the account of a supposed idiopathic excitability in the sensorium and nervous power, from contagion, when it ought rather to have been considered as symptomatic ? As illustrative of these conjectures, I will here put down the peculiarity in the vascular structure of the brain, as being essentially connected with most of its organic diseases.

147. The blood is sent from the heart to the head in the

usual manner, viz. by the carotid arteries and their ramifications, but its return is not the same as in any of the other parts of the animal machine, for it is propelled back by the medium of sinusses, into which the small veins empty themselves, and for the most part in a direct contrary course to that of the motion of the blood in the said sinusses. It is further worthy of observation, that the venous blood in the sinusses is thus deprived of the assistance of a continued muscular action, to propel it onwards in the manner it is in all other parts of the sanguiferous system.

148. Hence some sensorial and cerebral disorders may probably originate, in a preternatural distension of the sinusses only ; and from this cause, an accumulation of blood in the vessels of the brain is very apt to take place, especially after the middle age, or towards the decline of life, when the action of the heart and arteries, on which the circulation in the brain materially depends, becomes diminished (138). Earlier in life, affections of the brain may depend more on the hæmorrhagic efforts of the arterial system (135).

149. From these considerations, there can be nothing more obvious, than that the venous system of the brain is particularly liable to an accumulation of fluids, and to a plethoric state ; and more especially when concurrent causes operate, to obstruct the free course of blood from the head. I therefore apprehend, that a temporary congestion of the head, whether arterial or venous, may under certain states of the atmosphere, and certain irritations of the nervous system, lay the foundation for local inflammatory affection, when little suspected to be threatened or endangered. Yet it is very difficult to determine, when it is arterial, and when venous : but it is admitted to be most frequently the former.

150. Sometimes tumours within the cranium have been found on dissection, and have accounted for the great disorder of the system during life, most commonly of the chronic kind, but sometimes attended with a considerable degree of febrile derangement, not from having acted by a general compression

of the brain, so as generally to have affected the origin of the whole nervous power, but by a local action causing a local congestion, and thereby a general impression, or irritation, on the whole of the muscular or moving powers.

151. Dr. BAILLIE has observed in his “Morbid Anatomy,” that the membranes of the brain, as well as the substance of it, are variously liable to inflammation; but that the latter is not very common, when no injury has been applied to the head. He has, however, observed one thing in point, “that a partial separation of the *tunica arachnoides* from the *pia mater*, by a gelatinous fluid, is not an uncommon appearance of disease, particularly after fevers, in which the brain had been a good deal affected.” Were these epidemic fevers, or only the supposed contagious typhoid fevers of our own clime? It has certainly not been the practice with professional men to examine the *bodies* of those, who have died of typhoid, or contagious brain fevers.

152. Disordered states of the brain are generally characterized by peculiar affections of the eye, but many of them are symptomatic of constitutional diseased states. The injury done to the *retina*, by inflammation, causes the *intolerantia lucis*, affecting thereby the tendinous expansion of the organ of vision. It was just now observed (151), that the phrenetic febrile delirium, or inflammation of parts within the cranium, is most frequently an affection of the membranes of the brain, and but seldom of the cortical or medullary parts of it. The symptoms were observed also by Dr. CULLEN to be, diagnostically, very dubious in this disease, and he only pointed out, as characteristic of the phrenetic affection, “the *delirium, ferox*, and *suffusio oculi*.” He had also added the *typhomania* of SAUVAGES, because it took in all the symptoms of maniacal delirium, which are met with in the PHRENITIS; but that is evidently a different organic affection, because, as he observed, “typhomania is best cured by opium.”

153. As sometimes connected with slight phrenetic affections I shall speak of the *erysipelas*, an exanthematous dis-

ease (122). This class, the *exanthemata*, consists of diseases, which are mostly epidemic, but the erysipelas is generally sporadic, (57) and of a relative character to the phlegmasiæ. It begins commonly at the ear, and creeps along the face and hairy scalp, and will be sometimes accompanied with a tendency to delirium. This has been supposed occasioned by an effusion of morbid serum running along the vessels; but Dr. CULLEN doubted, that such a morbid translation to the membranes of the brain took place. I believe it more consonant with fact, and the prevailing opinions of this day, to suppose, that the internal affection of the brain arises from simple communication of inflammation, by the ordinary circulation of vessels. Being so often immediately concerned in occasioning some of the morbid states of the brain, I shall next introduce, as direct causes of death, whatever may produce either a compression, or a destruction of the texture, of the medullary substance of the brain.

154. Compression certainly in many cases proves the direct cause of death, and may be considered as fairly destroying the excitement; though it has been more frequently thought an indirect cause, and been conceived to act only by destroying the functions of those nerves, that supply the vital organs, and not immediately life itself. It was on this ground, that CULLEN entertained the opinion, that *coma* in fevers was no proof of a compression of the brain, but rather of a state of collapse in the nervous energy. “Had it” said the Professor, “been the effects of compression, it would have always proved fatal; but as this was evidently far from being the consequence of coma, I therefore look upon it as a temporary collapse.” He seems hereby to have admitted no medium state, or degree of inflammation of the brain, as causing compression, without being necessarily fatal.

155. In regard to the destruction of the texture of the medullary substance of the brain, as relative to febrile affections, I think it is to be questioned as a direct cause. That various

topical affections of the brain take place in fevers is undoubted : but a destruction of texture more frequently arises from blows, and other accidents, occasioning injury to the brain itself. These indeed, by their irritation, may produce convulsions, which may destroy the patient : under such circumstances they are certainly indirect causes. Dr. CULLEN therefore considered the only direct causes of death in fever to be, either increased excitement, or poison : but the latter may surely be considered, as often an indirect cause.

156. The indirect causes of death more immediately act upon, or affect, the circulating system, and the various functions of the animal economy connected with the heart ; and when they produce fever, with or without a conjoint operation on the brain, are all referable to one or other of the three following heads, according to their several modes of action, namely, to the excess of the stimulant, the excess of the sedative, or the excess of the septic power.

157. The pulse becomes particularly characteristic of the nature of the fever, when connected with morbid affections of the brain. The strong pulse is consequential to sensorial, or cerebral, irritation, and inflammation, and is always to be considered as a mark of the inflammatory diathesis. The slow pulse is not always a proof of the absence of irritation from local cause ; for in some persons the pulse will be found slow, when they are in a comatose state ; but then it is to be supposed, that there is some compression on the brain, which prevents cardiac irritation (78). Thus the frequency of the pulse, which so much characterizes most other pyrexial diseases, is not, however, so striking in symptomatic fever accompanying topical affections of the brain (79).

158. The inflammatory diathesis, when attended with a determination of blood to particular parts of the system, namely, either to the head, chest, or abdomen, may become in its consequences very dangerous ; but this determination is never more so, than when it is made to the head, from the

operation of occasional causes often so much favouring it. Of this topical affection I shall therefore speak more particularly.

159. The particular determination of blood to the head, is most common to all fevers, and is most apt to be considerable; the cause of this it is difficult to assign, but it was enough for Dr. CULLEN's purpose, that the fact was generally admitted, and he thought proper to distinguish it by the following symptoms. It is to be apprehended, said he,

I. When a peculiar throbbing, and pulsation, occur in the carotid arteries, near the spheroid bone.

II. When more or less of a considerable redness appears in the face, with some degree of turgescence. This however may accompany the beginning of all sweats.

III. When the redness extends to the eyes, from the vessels of the *tunica adnata* readily filling, and becoming universally distended on such occasions, which can be anatomically accounted for, from their connection with the above carotid arteries (146 et seq.).

IV. When, though not always, yet in many cases, an increased sensibility to light and noise takes place, more especially when accompanied with the foregoing symptoms (152).

V. When head-ach occurs with the other symptoms, for it then may justly be deemed a mark of the determination; but it is to be considered a doubtful symptom, because it has sometimes been attendant on the cold fit of fever.

VI. When constant watching, and disturbed sleep occur; and these are to be reckoned the most dangerous and strongest symptoms of an increased and alarming determination to the head.

160. Such have been described by Teachers generally, as the ordinary marks of a determination of the blood to the head in fevers; but they have been most strongly noticed, as characterizing the phrenetic delirium, which is always attended, when violent, with constant watching, and furor in the tone of mind, inflammation of the face and eyes, restlessness, and a

constant inclination to get out of bed; and a confusion of ideas. All these symptoms, when more or less combined, indicate a real inflammation in the head; and though the patient has in many cases recovered, yet, for the most part, such symptoms of determination to the head prove fatal.

161. Dr. CULLEN observed, “that the phrenitis, when purely topical, was very certainly mortal.” And he also further observed, “that death is almost certainly foretold in fever, when, in concurrence with the symptoms of general debility, either topical inflammation of the brain, or the highest degree of putrefaction, is present (106). He particularly noticed one very important observation of the late Sir JOHN PRINGLE, “that upon every dissection of such, as died of the gaol fever, he found abscesses in the brain,” which, said Dr. CULLEN, “was a certain proof that it had been topically affected.” It redounds very creditably to the memory of Sir JOHN PRINGLE, that he pursued the investigation of the disease so far, as to examine the head after death: but still the disorder was considered an idiopathic fever.

162. Consistent with the extent of my views, which will hereafter more fully appear, I shall here introduce an enumeration of those symptoms, which, together with the symptoms of general debility, were considered by Dr. CULLEN in the typhus, as certain presages of death, and indications of topical affection of the brain. They were;—to wit, topical irritation of the brain—great sensibility of light and sound;—increased action of the heart and arteries;—suffusion and redness of the eyes, with flushings of the face; and sometimes topical affections of the lungs and abdomen;—coma-vigil, inquietude, and restlessness. Danger is equally portended, if, upon a low delirium, any of the phrenetic symptoms supervene, together with a small contracted pulse;—irregular spasmodic, convulsive, tremors;—subsultus tendinum;—spasmodic motions of the eyes;—&c. I should myself consider any pulse, of the nature of the above described, if exceeding 120 beats in a minute, as a mark of debility with irritation; and certainly

appertaining to the proper typhus fever more than to the phrenitis. Limpid urine has been mentioned by some writers, as a sure sign of an approaching febrile delirium.

163. Several important remarks may be made on these various symptoms, as pertaining to the object of my future enquiry, which is, generally speaking, to establish a more clear and decided pathology, if possible, of the ordinary epidemic, and apparently contagious, disorders of the Continent, and tropical climates, in the hope of either lessening their frequency, or of establishing a more successful method of cure. If I should succeed in provoking a further enquiry in the minds of those, who, as able and experienced physiologists and pathologists, may be more qualified to establish the views I have had than myself, I shall feel gratified by the humble attempt I here submit to my professional brethren. But the perfect elucidation of this important subject must rest, ultimately, not only on the reflections of those intelligent practitioners, who have already witnessed the scenes of pestilence, but of those also, whose fortune it may be in future, to be engaged in the treatment of the malignant pestilential disorders of tropical climes.

SECT. IX.

ON THE IMPAIRED ENERGY OF THE SENSORIUM AND NERVOUS POWER IN FEVERS.

164. From the same motive (128) I shall next proceed with some important observations connected with sensorial pathology. Dr. GALL, of Vienna, in the paper before alluded to, (Med. and Phys. Journal, No. 80) has observed, that “the anatomists and physiologists almost of every age endeavoured to investigate the *direct* seat of the soul, and to find out the very spot *wherein* the different faculties of the mind are resident.” But the Doctor, on the contrary, “has considered

the brain, not as the common organ of the soul, but as *an assembly or an aggregation of the different organs.*” If future anatomical investigation should establish this doctrine generally, it may lead to a more pointed and successful treatment of the various local affections of the brain, as distinctly referable to one or more of his thirty-two ORGANICULI ENCEPHALI. For our present purpose, it will be sufficient to speak of the brain in more general language, as the aggregate organ of the mind and senses.

165. The affections of the sensorium and nervous power are characterized by intellectual changes; by pain and muscular irritations; and by inordinate actions; varying according to the degree and extent of any particular local injury or defect. Delirium and convulsion are the striking features of sensorial derangements. The particular symptoms referable to the intellectual functions, as excited or altered by fever, appertain more particularly to the doctrines of physiology: or rather, as under morbid changes, are then referable to the science of pathology. They depend directly upon the nervous power alone, which varies, giving rise to the denominations of excitement and collapse.

166. Consistence and coherence in thinking depend upon a certain equality in the excitement of the nervous power, which constitutes the true and natural sensorium; and delirium, or a deviation from the above, is owing to some alteration, or inequality in it.

167. Delirium is of two kinds, or degrees; the *delirium mite*, or low, and the *forte*, or violent *delirium*. Coma and the delirium mite in fevers are ordinarily considered, as depending on collapse: they certainly arise also from compression. But the delirium forte, or high phrenetic affection of the sensorium, depends upon increased excitement.

168. Delirium and coma may be produced in fevers, independent of the general state of the circulatory organs, or a morbid action of the heart, and on such occasions must arise from topical affections of the brain itself, or its membranes.

169. Delirium, in a greater or lesser degree, is an ordinary attendant on fever, and follows the general affection of the system. That it is frequently connected with the state of the circulation, appears to be proved, by its generally happening in the hot fit, or during an exacerbation, attended with head-ach, throbbing pulse, &c. (32, 45). It may therefore be fairly suspected, that febrile delirium is owing to an encreased impetus of blood, or altered state of circulation in the vessels of the brain itself. This was Dr. BOERHAAVE's opinion; but CULLEN went farther, and attributed delirium to a change in the nervous power, or substance of the brain itself, on which he supposed this alteration in the intellectual functions depended.

170. Febrile delirium differs in its character, as well as in degree. Teachers of medicine have said, that when it happens during the hot fit, or exacerbation of fever, it has been supposed to arise from excitement; and that when it occurs in the cold fit, it is then considered as occasioned by a collapse arising from debility; and is said then to resemble approaching sleep. I should, however, suppose, that temporary compression from congestion might, in some degree, contribute to produce it during the cold fit (165).

171. CULLEN and BOERHAAVE differed in their theories respecting sleep and watching. The former thought them dependent on the varied energy of the brain, while BOERHAAVE imagined them to arise from certain diurnal changes in the circulation of the brain: they both may be concerned with Dr. KELLIE's opinion, "that respiration has a connection with the vitality of the brain." (133). The alterations in them, caused by fever, have been considered as effected by that debility or collapse, which induces delirium; and on this ground CULLEN founded the above opinion, and felt very confident, that the leading causes of disease always depend upon their mode of action on the state of the nervous power.

172. In as far, therefore, as that violent excitement may become the cause of diminished excitement and general debi-

lity, I think it probable, that it might be the sole leading cause of fever; and in this way we might account for those intermittents and low nervous fevers, which in the metropolis cannot be traced to any source either of the marsh, or human, effluvia, or of any direct contagion. The leading remote causes of such fevers have already been generally spoken of, (57 et seq.). I shall confine myself in this place to a very important one, particularly at the times of prevailing pestilence, from its immediate relation to the nervous power. I speak of FEAR, an intellectual action or impulse of great effect on the system at large: but before I enter upon the promised remarks, on its operation generally, I shall point out some corporeal actions and alterations, with which the low nervous fever is especially attended, though not always accompanied with delirium.

173. In general the expressions of the face, and the motions of the eyes, are the index of the soul, and consequently of the energy of the brain. Their healthful state or action however, becomes affected by fever, and then, by the muscles of the face being without motion or expression, the eyelids half raised, and the eye itself regardless of objects, the whole countenance marks the general debility of the system. These are the proper symptoms of impaired voluntary motion, from the diminished nervous energy of the whole system.

174. When the debility is very great, general tremors take place, and some are observed in the countenance by the motions of the lips, tongue, and lower jaw; in the increasing state of it, spasms and convulsions will occur. These, however, CULLEN admitted, as sometimes arising from debility, and sometimes from irritation, and observed, “that though in fever they are evidently owing to debility, yet that they certainly may arise from irritation, in consequence of a certain degree of topical affection of the brain;” they are also frequently connected with some change in the state of the mind, which is often so *apparently* affected through febrile debility. I shall therefore relate, as briefly as possible, the several affections of the animal economy, which indicate debility of the sensorium,

as generally noticed by teachers of medicine, and as strikingly characteristic of some *pestilential* diseases.

175. The common tone of the mind is first affected by a state of dejection and despair; this symptom of debility is observed to happen to the strong and athletic man, as much as to the weak and delicate woman, so as to render him equally timid: it is not always to be attributed to some moral cause of anxiety. In the next place the intellectual faculties become affected, through *apparent* febrile debility. The affection of them is marked by difficulty of recollection, want of a proper association of ideas, and a greatly impaired memory. It will be at the first expressed by dreaming: the dreams are generally of the melancholic and frightful kind; they interrupt sleep, and are attended with a certain confusion of thought. As the latter increases, an actual incoherence takes place at the time the sick person is about to fall asleep, and when he awakes is not able, for a considerable time, to recollect himself. Moreover, when a patient is thus affected, while he is quite awake, he may be said to be in a state of actual delirium. This species of delirium has none of those symptoms, which attend on the commonly called true phrenitis; on the contrary, the patient is mild and calm, and will sometimes have ideas of the cheerful kind; but the phrenetic delirium is accompanied with more of furor, anxiety, restlessness, &c.

176. Another state of the sensorium to be noticed is the *coma-vigil*, or constant watching. This has been reported a symptom of either increased impetus of the blood in the brain itself, or of an incipient state of inflammation, probably of the membranes only. The alteration, or disturbance, of sleep may, on most occasions, be consequential to various other topical affections, which cause local irritation, and CULLEN owned, that at the first the brain might be thus slightly affected, and yet a phrenitis ultimately ensue. But *coma-simplex*, or constant sleep or drowsiness, is a symptom of remarkable collapse, and debility of the system; though CULLEN conceived,

that it might be owing to a combination of irritation, and debility, arising from some alteration in the action of the excitement, or collapse, in consequence of the commencement of some topical affection of the brain; yet he generally considered the coma-simplex, as depending chiefly on debility.

177. There is a species of coma attended with still less irritation, wherein the memory is entirely impaired. This is the proper lethargic state. The degree of oblivion, or loss of memory, is so great, that the patient knows not where he is, nor his most intimate friends and relations. In short, he becomes insensible of all external and internal impressions, and in this state voids his urine and fœces involuntarily. These consequences are truly to be attributed to his insensibility, and not, as too often has been fatally supposed, to general debility, or a paralytic affection of the sphincters.

178. It may be further observed, that from the same general cause, namely, the impaired state of the nervous energy through the system, the heat of the skin becomes very great, the tongue dry, and yet the patient has no thirst, nor is he able to distinguish the particular fluids offered him, and at the same time the sight and hearing are impaired. These were, in Dr. CULLEN's opinion, certain symptoms of a weakened nervous energy. To the same sensorial state has been attributed the patients seeing double, or imaginary objects before him, such as spots or prominences, which he is constantly catching at, and endeavouring to remove.

179. The effects of a general nervous debility, as connected with the state of the sensorium, are also characteristically pointed out by certain changes in the circulation, in the respiration, and the alimentary canal: but of these I need not so particularly speak at present, yet shall observe, that the stomach, the organ of the body of the most exquisite sensation, shews always a very considerable sympathetic connection with the morbid states of the sensorium (34). It is also influenced by other local affections. STAHL and CULLEN agreed in opinion, that the gout did not depend upon a morbid matter,

but originated in a derangement of the nervous power; the latter therefore observed, that the sensorium seemed to be as it were *distended*, or in some way topically affected, on the approach of the gout, by a remarkable dejection of spirits, by an aversion to study, and a kind of confusion in the head. Of such a *sensation* in the head, (not however connected with gouty diathesis,) I have been myself sensible, when I have been for several hours together reflecting, and writing on the subject of this work; on quitting it, the distended sensation in the head has in a short time left me.

180. There are causes, which may be said to affect the nervous power by mechanical action, independent of compression from congestion, or effusion. Thus the case of ordinary sleep happens without any compression, as is proved by people falling asleep, when you take off the action of external stimuli. But a morbid degree of sleep may arise from causes, which in some manner act upon the sensorium, in the same way as they produce the apoplectic state. In this way, by altering or diminishing the natural state of excitement, do extreme cold, the steams of strong fermenting liquors, electricity, various causes of sensorial concussion, violent passions both of the active and sedative kind, occasionally operate, through their effects on the brain. It is now time to enter upon the intellectual operation of fear, as a remote cause of fever, especially when epidemic (172).

181. The sensitive power of the system has appeared on some occasions to have been so wonderfully acted upon by disease, that the principle has given rise to the introduction of charms in the cure of intermittents, and some spasmodic affections, as the hooping-cough, epilepsy, hysteria, &c. On the same ground antidotes to the plague, without any specific power granted to them, have been approved of, as fortifying the mind against its epidemic power. Surely therefore, if the mind can be so influenced on these several occasions, how great must be the operation of fear on the human frame, in raging pestilential seasons (69).

182. FEAR, or fright, has been known, from its sudden action, to have so deranged the balance and connection between the sensorium, and circulating system, as to produce an irregular or premature return of what had been the regular paroxysm of an intermittent; and so intimately connected are the nervous and circulating systems, that I believe it fair to attribute the varieties of intermittents, both as to their duration and species, to the varieties in the nervous temperaments of different subjects (22 et seq.). CULLEN was of opinion, from the communion of the nervous and vascular systems, that the major part of head-achs might be deemed topical fevers (45).

183. Terror and sudden fright have also much influence both in producing, as well as aggravating, continued fevers. From their acknowledged debilitating power they are said to have probably occasioned, in some epidemic seasons, cases of the typhus, and low nervous fever; but for the most part they are assisted by the morbid operation of cold, and other concurrent remote causes. From their instantaneous and sudden effects, or action, on the heart as well as on the sensorium, it is rational to suppose, that they produce such an immediate change in the circulation of the encephalon, through the brains sympathetic connection with the heart, as to lay the foundation of *local mischief* in the first instance, *and to add it*, when the general febrile derangement of the system has already taken place, from the other febrile causes generally prevailing.

184. Fear, however, may operate in different ways on the system. Thus, it may, by increasing the impetus of the blood, cause menorrhagia or abortion; on the other hand it may occasion a suppression of the menses, by producing constriction. A difference in the nervous temperament must be the cause of this its variable mode of action on different women.

185. The powerful effects of fear, or rather terror, as occasioning nervous and spasmodic affections, merit particular notice in tropical practice, being immediately connected with the state and condition of men, at the period of any raging epide-

mic. It may be considered as that passion, or impression on the mind, which arises from the view of an immediate and great evil suddenly presented to us. Every case of uncertainty is, in some measure, a cause of fever, as appears from the expressions children will make in the dark; and as every new and unusual object, is an object of uncertainty, it is productive of these effects on the mind, especially if the object be very large in bulk, or of intense force, or disagreeable in quality, and above all, if of acknowledged fatality in its consequences, especially if the knowledge of it is suddenly presented to our view, or mind (68 et seq.).

186. Fear, therefore, according to its nature and degree, operates very differently on our system, but generally as a sedative passion. As such, in its highest degree, it has produced palsy, apoplexy, syncope, asphyxia, and even death. In a certain degree, however, it is said to have acted as a tonic, in the cure even of some epilepsies; I should rather think as an antispasmodic also: but as we do not always know the proper method of exciting it, nor the measure of it, which possesses this quality, we therefore can seldom have recourse to it, as a probable means of cure. These facts prove the important connection of life and health, with the immediate actions, or exertions, of the brain itself (22). But as the force or application of terror varies, so its principal effects are not in all cases, or constitutions, precisely the same.

187. There is no doubt, but that fear does sometimes operate principally on the sanguiferous system, that is, on the heart itself, and from thence arises the sense, or feeling of palpitation; it may also affect the arteries, and thus produce stagnation, and congestion in different parts of the body. In this way a turgescence in the vessels of the brain may cause palsy, apoplexy, and epilepsy. Dr. CULLEN took notice of a particular case of epilepsy, produced by fright, the fits of which were always preceded by head-ach, pain over one eye, with an inflammation on the tunica adnata, which indicated some congestion of the head. It may therefore, from these

premises, be fairly concluded, that though fear generally acts by collapse, yet that it may sometimes produce irregular motions in the sanguiferous system generally, and thus occasion some partial and sudden congestion of the head, and that a morbid mobility, from turgescence of the system, must surely have some connection with febrile delirium (135, et seq.).

188. From the generally known fatality of the continental and insular fevers of the western hemisphere, all visitors from this country, as well as from every other, must arrive with dread in those quarters of the world, from its having been so universally observed, that as new comers, they are particularly liable to the endemic diseases of those climes. I shall therefore here introduce the particular observations made by Dr. MOZELY, as pertinent to my subject, on his concluding the account he has given of the climate of the West Indies: for the yellow fever will appear *hereafter* to have been, most frequently, of a relative nature to the Gibraltar disease.

“ I cannot,” says this experienced author, “ dismiss the present subject, in which my views have been principally directed to the avoidable and remediable derangements of the body, without one solitary glance at those derangements of the mind, which no regimen can prevent, no medicine cure.”

“ Hot climates administer certain death to a *mind diseased*, and where there is in the *memory a rooted sorrow, or written troubles of the brain*. The want of sleep in the slightest indisposition is always alarming, and in the graver diseases, of people who have naturally much irritability of habit, or some grief, or anxiety of mind, it is the cause of so great a determination of blood to the head, with excessive action of the arteries of the brain, and so much perturbation of the animal spirits, that often admit of no relief or composure, but what the unhappy sufferer, after the most violent convulsive struggles, phrenzy, and inflammation of the brain, finds in death.”

“ Sometimes he escapes this fate, to experience the miserable alternative, of a long imbecility of the faculties of the mind.”

“ Therefore,” says the Doctor, “ let not the discontented in

mind, nor the broken hearted, hope to evade his cares and troubles, by changing to these climes; nor think that any passion which has stormed the breast, will abate its force by distance.—NOSTALGIA,—that longing after home, exerts its painful influence in the remotest regions, and magnifies to danger the most trivial indisposition of either mind or body, when both are already half subdued by the heat and dread of climate. Those, whose happy days have not yet been clouded by misfortunes, let them be careful *here*, to observe tranquillity of mind, and watch with caution over their passions. The young and inexperienced, who have embarked with the false notion, that fortune has heaped up treasures for them, to be delivered out *gratis*, let them also prepare for disappointment: and let them avoid, at first arriving in these countries, entering into any serious engagement, or intricate concern, until they have made themselves acquainted with the genius of the people, and their local laws, for fear any glittering allurements should lead them into an inextricable labyrinth of difficulty and vexation, and consign them “*to that country, from whose bourn no traveller returns.*”

SECT. X.

ON CERTAIN MORBID CONDITIONS OF THE BRAIN, IN SOME ACUTE AND CHRONIC AFFECTIONS.

189. I shall conclude THE FIRST PART of my undertaking with some further observations generally connected with the preceding sections, and my ultimate enquiry; and with the intentions I before declared (128). We may justly attribute to the influence of the SENSORIUM, and particularly to that NERVOUS POWER, of which I have just said so much, the exertions and effects of that wonderful principle in the human frame, which certainly exists, though some have denied it, denominated in the schools the “VIS MEDICATRIX NATURÆ.” or as ex-

pressed by other words, “*the certamen inter naturam et morbum*” (83). The doctrine of morbid matter has certainly been very scientifically exploded of late years (179), and a more rational principle has been established, namely, that the nervous power has the greater share in the production of fever; and that the event must be determined on, by observing those motions, which may prove the means either of restoring health, or inducing death; but the difficulties, which have ever stood in the way of satisfactorily explaining the causes of life and death, will always embarrass us in our endeavours to establish a sure and certain prognosis (23 et seq.).

190. I think, however, that the weight and influence of a *vis medicatrix naturæ*, in the animal system, is mostly evident, and brought to bear under disease, when the sensorium, or seat of the nervous power, is only secondarily, or symptomatically concerned. This healing principle may very readily be admitted in local affections of the system of the inflammatory character, the seat of the sensorium itself alone excepted: or in low nervous fevers arising from personal uncleanness, or other human or epidemic effluvia. But in fevers connected with local affections of the brain itself, I believe that principle, the *vis medicatrix naturæ*, loses a great portion, if not the whole of its salutary or healing power and actions, and may even, by the exertions it may make, increase the danger attending on all degrees of sensorial inflammation.

191. The debilitating power of fever, particularly if accompanied with cerebral inflammation, is so great, that it will immediately reduce the strongest man to the weak condition of a child, and sometimes almost to the state of approaching death. This must have its foundation in the disturbance of the nervous principle of life (71). When such sudden effects take place during a truly malignant epidemic, it may be owing to the strong power of the sedative miasma on the brain; and in this way we may account for the sudden deaths during a plague, from pestilential vapours having the power and effects, as teachers of medicine have supposed, of poisons on the human senso-

rium (174). Dr. CULLEN observed, “that whatever might be the true nature of the animal poisons, so frequently deleterious to human existence, they clearly appeared to act quicker or slower, according to the quantity, or quality, at last produced in the system by a supposed fermentative process, when their sedative power is not at first so great, as to kill suddenly.” If, however, I should ultimately prove right in the opinion I have taken up, respecting pestilential fevers, the doctrine of fermentation may be fairly laid aside.

192. Of all the diseases to which the brain is liable, the *phrenitis vera idiopathica* is the most important, as well as the most rapidly fatal; difficult also of detection in its lesser degrees, and I think not so rare as may be generally imagined. From the frequency of mental derangement during fever, I think this local disorder merits the most serious consideration. CULLEN went so far as to declare, that he had only once, if he had ever, met *with it* in the course of forty years practice. He observed, “that it could only be said to happen, when the redness of the face and suffusion of the eyes, together with the delirium ferox, were coeval with the attack of the disease.” He also thought, “that all writers confounded the symptomatic with the idiopathic phrenitis.” In this latter opinion I think he was mistaken, on pathological grounds.

193. It is, in my opinion, of the first consequence in febrile practice, early to discover, even to suspect, the existence of such a local affection: for whether coeval or not with the attack of fever, that is, the *pyrexia*, it must at any period very considerably add to the danger the patient must be in. The successful treatment of sensorial inflammation must very much depend upon its being known, to be either the idiopathic, or the symptomatic, I shall, in this place, only briefly notice what most particularly appertains to the cure of the true idiopathic phrenitis.

194. Venæsection is the principal remedy, immediately that it is suspected, and that too very freely. Topical bleed-

ings are to be preferred, as the opening the jugular veins, or the temporal arteries : but this last is often attended with trouble and inconvenience, and may be superceded by scarifying and cupping the temples (87). When caution in brain fevers is necessary respecting venæsection, purgatives are well adapted to take off the inflammatory diathesis, and lessen local inflammation ; but they are particularly so in the case of topical inflammation of any part of the brain, by the revulsion they are supposed to cause (123). Blisters are of the greatest service. It was the practice of the ancients to apply them to the most remote parts ; but at this day we know that the nearer the seat of inflammation the better (89). The whole of the antiphlogistic regimen ought to be attended to.

195. Of internal remedies I shall only take notice of two, nitre and camphor, the union of which, as a general medicine, would be found serviceable in most phrenitic affections. Camphor has been sometimes used in external inflammation ; but not alone in internal ones. When nitre is added to it, it may be found of much advantage in some cases of cerebral inflammation. But, as Dr. CULLEN observed, “ when the sensorial affection was more of the maniacal than phrenitic character, camphor alone, in a full dose, had the happiest effects in procuring rest (105).”

196. There are some similar affections, which certainly do not depend singly upon an accumulation, or inflammatory obstruction of blood about the head ; because phrenitic delirium often accompanies other affections, particularly the *peripneumonia*, or inflammatory condition of the body or substance of the lungs, attended with a suffusion of the cheeks, from difficult transmission of blood through the lungs. The inflammation of the diaphragm, denominated the *paraphrenitis*, has been so called, from its having been commonly supposed attended with delirium ; but it is not always so : while, on the other hand, in some other affections of the thorax, it will sometimes take place. See Morgagni de Sedibus, &c.

197. When we consider the minute and complicated structure of the brain and its appendages; can we be surprized, that the human frame should be subject to such a variety of diseases, which arise from causes often latent, disturbing and deranging the sensorium and nervous system, without any evident local disease connected with the hydraulic part of our machine, the circulation, and apparently unaccompanied with fever? Such diseases would seem to depend upon a weakness in the functions themselves, however connected with the nervous system; or from irregularities in those functions flowing from some organic derangement. The brain and nervous power being the fundamental parts of the animal economy, from its primordial existence, (132) there is even much cause to wonder, that we are not more exposed to sensorial disorders than we are: but that we are, oftener than suspected, is my firm opinion: for, the proper origin of the nervous power is seated in the tender medullary part of the brain itself, as well as of the cerebellum and medulla oblongata; organs of the most tender, and complex structure (129).

198. Again: there are a variety of diseased affections connected with the nervous system, but operating chiefly on the muscular system, by inordinate actions. These must depend on a certain action in the brain itself, which action is determined by a certain measure in force, velocity, &c. and for the most part the several actions of the system, and that of the brain itself exciting them, are separately exerted. Spasms, epilepsy, convulsions, and diseases of that character, are to be attributed to a derangement of the sensorium, as depending on the irregular exertion of the brain itself (164 et seq). The causes of violent excitement, when instantaneously acting on it, will induce such diseases; for example, the force of anger, &c. I shall accordingly put down a few observations on diseases particularly connected with a morbid state of the sensorium, leaving my reader to make the application of them to such parts of the preceding general pathology, and the

intended investigation, as in his judgment are most appropriate to them.

199. APOPLEXY may be divided into two distinct species, the hæmorrhagic, and the hydropic, or in other words, the sanguineous, and the serous. The first species is most successfully treated by bleeding freely from the jugular; but it requires, as before alluded to, much *acumen* to distinguish, when it arises from arterial, and when from venous, congestion (148); in either case the bleeding might do good. Blisters to the temples were strongly recommended by Dr. CULLEN; but he thought them hurtful in the hydropic apoplexy. Why he thought so I could never comprehend; he in this species preferred purgatives of the acrid kind, from the great evacuation they produce: why should not therefore blisters in the hydropic case, even to the whole head, relieve locally, as well as arteriotomy in the hæmorrhagic apoplexy? When it is a clear case of hydropic apoplexy, I think, they must do good, particularly when arising from the state of collapse; the cases of true apoplexy from collapse, are caused by either poison, electricity, or cold.

200. In regard to PALSY, I find occasion to make but a few remarks, it being known to originate from most of the causes of apoplexy, and is often immediately connected with it. Its affinity therefore to it seems well established. The loss of sense accompanying palsy depends perhaps more on the loss, or impaired state, of circulation in the part affected, than in the loss of nervous energy. As connected, however, with our hydraulic system, the circulation, the subject remains very obscure, from its variability in degree of fulness, impetus, and determination. The intellectual faculties are generally unaffected in palsy, but sometimes are, particularly when arising from tumors within the encephalon, occasioning partial compression, and sometimes an imbecility of mind is owing to a state of collapse subsequent to apoplexy. DIEMEMBROEK mentions the case of a girl, who through a violent fit of fear was seized with hemiplegia, affecting the lower extremity on

one side: it continued thirty years, and was suddenly removed by a flash of lightning.

201. A morbid state of the brain is also one of the most frequent causes of EPILEPSY. This state is generally a turgescence of its vessels, which has appeared manifest on the dissection of persons, who have died of epilepsy, and who had laboured under the various symptoms of præternatural congestion in the brain; it may be difficult to determine whether this was the cause, or the effect. Many symptoms, however, of inflammation, and increased impetus, have also been found on such examinations; and also the marks of purulency, which never takes place without previous inflammation, and increased impetus (161). Dr. CULLEN also observed, that the phrenitis, which is most certainly the consequence of inflammation, has very often been known to attend on epilepsy: epilepsy may therefore be considered as bearing some affinity to apoplexy, in as far as it is very frequently the consequence of increased impetus, and in this manner we may account for intoxication, anger, and violent exercise, inducing epilepsy.

202. Chronic affections of the sensorium have been characterized, as not attended with fever or coma. They chiefly regard the state of the intellectual faculties, and are not very easily defined (165). The distinction between sense and madness, can best be learned by an application to common sense (166), and will admit of division according to the manner of their affecting us, sleeping or waking (171).

203. FATUITY is supposed to arise from flaccidity of the brain in infants, and from rigidity in old age. However, it is so difficult to reduce the subject to a just and clear pathology, that this mental state has generally not been looked upon as an object of practice. A firmness in the medullary substance of the brain has been observed by myself, on the examination of those, who had been subject occasionally to fits of the epileptic kind.

204. MANIA and MELANCHOLIA are the mental diseases, which become more the object of practical investigation, and arise from the inequality of the excitement, or healthful action of the

sensorium itself (169). Maniacal persons powerfully resist all sedative impressions on them, from the force or power of the cerebral excitement; but the febrile delirium admits more clearly of relief, from its being known to arise either from the increased impetus of the blood, or the unequal excitement of the brain, from local plethora or obstruction.

205. Various are the direct causes of those states, which give rise to the above mental derangements, as tumours, &c. within the encephalon (150); but the ordinary remote causes are sudden and violent passions, as fear, joy, &c. All long and durable passions; as grief, disappointments, intense applications, especially to one subject: these are the mental causes; but such affections of the sensorium occasionally take place through the body, sometimes from poisons, sometimes from fevers, suppressed evacuations, repelled eruptions, &c. A caution is here important: from the analogy between mania and phrenitis, beware of the use of opium, with the least tendency to local congestion, or the plethoric state (102 et seq.).

206. Those who would wish to know more on this subject, I beg leave to refer to the writers on maniacal affections, and mental derangements. We have had within these few years, a very scientific and instructive work on such sensorial disorders by ALEXANDER CRICHTON, M. D. at present Physician to the Emperor, and the court of Russia.

POSTSCRIPT.

On a recapitulation of the subjects of the several preceding sections, I think I need say little more now, than what will tend to impress my reader with the importance and propriety of premising them, as the ground-work of the intended investigation.—I first called his attention to the advantages of a dogmatic plan of education, because such an illustration could not be either satisfactorily attempted, or readily understood, without it.—I next offered a few observations on our primordial existence, and on the principle of life, that it might be the

more easily comprehended, on what foundation our health might be deranged, or death occasioned, through accident or disease.—After those brief remarks, I next judged it expedient, that the leading causes, symptoms, and varieties of fever should be, in a general way, noticed, that the ground of my deviating from the opinions hitherto entertained, might be hereafter the more readily admitted, or rejected.—It then naturally followed, that I should briefly speak of contagion, cold, and fear, as the universally admitted, causes of a spreading febrile pestilence.—These points would of course lead me on to make a few remarks on the state of the atmosphere, on the variability of the pulse, and on the changes in animal heat, as connected with, and affected by, the febrile remote causes.—In the next place I found it necessary, in order that the intended deviation from the hitherto admitted principles of practice, which I shall have in proper time to submit to my reader, might be the better understood, that I should, in a compressed manner, lay before him, the present general method of cure in fevers, even of the pestilential kind;—and as the latter have been so much characterized by a particular tendency to the septic state of the system, I should have thought myself remiss, if I had not pointed out some of the leading circumstances marking an excess of it.—As this excess in pestilential seasons has appeared to the author to arise much from the prevalence of the inflammatory diathesis, aggravated by the state of the atmosphere, he was next led to introduce a few remarks on inflammation, and its terminations, as appearing so often connected with epidemic, and even with some truly contagious, diseases.—This necessarily called upon him to put down a few select observations on the virulence of epidemic fevers, whether arising from human, or atmospheric, effluvia.—When so far advanced in the subject, as forming the ground-work of the intended investigation, I was led, from observations in my own practice, and the impressions made by my readings on pestilential fevers in general, to think, that the virulence attending on them was in an especial manner accompanied by SENSORIAL AFFEC-

TIONS, in so much so as to establish, in a great measure, the danger, according to the degree of THEIR existence, particularly when noticed on the attack of the fever. I therefore thought I should well serve the general cause, as to the object of this the first part of my undertaking, and also aid my reader in entering with me into the much wished for investigation, if I laid before him some remarks on the established connection existing between the brain and circulation, under fever:—on also the impaired energy of the sensorium and nervous power, when disturbed by febrile actions of the system;—and also point out what have been suspected to be the morbid conditions of the brain itself, in some acute and chronic affections.

By reflecting well on the preceding general principles, as more or less connected with febrile pathology, I think the mind of the experienced practitioner will be gradually inclined to question the validity of former opinions, and be in the end disposed to coincide with the author in his belief, that MALIG-NANT PESTILENTIAL TYPHOID DISORDERS, particularly of the tropical climates, are in truth to be more considered as SYMPTOMATIC, than IDIOPATHIC, FEVERS.

END OF THE FIRST PART.

